

**A Safety Net for Sickness? The Effect of State-Level Paid Sick Leave Laws on
COVID-19's United States Spread**

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Abstract

Paid sick leave (PSL) prevents the spread of infectious disease by allowing workers to stay home and tend to personal and familial sickness. In this study, I investigate whether already-on-the-books state PSL laws in the United States significantly influenced social distancing, cases, and deaths during the early months of the pandemic's U.S. course. Leveraging geographic variation in U.S. state-level PSL mandates which were in effect since at least January 2020, I find that people in states covered by PSL mandates stayed at home for about 30 more minutes relative to people in states not covered by PSL mandates immediately following the National Emergency Declaration on March 13th, 2020. My results suggest that this increased distancing translated into fewer deaths and cases in states covered by PSL mandates, though the health outcome differences were not significantly different from zero. I also find that in states covered by PSL mandates, individual characteristics such as educational attainment and ethnicity were associated with differing levels of stay-at-home behavior: higher college-educated shares were associated with more distancing, and higher shares of Hispanic people were associated with less distancing. Overall, this thesis contributes to a growing body of work which studies optimal policies for supporting the health and well-being of individuals and families during a public health crisis. My results offer some evidence that paid sick leave mandates do achieve their intended goals of keeping sick individuals at home during a pandemic, but to a modest degree.

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I. Introduction

Paid sick leave (PSL) provides workers with the opportunity to tend to their short-term infections, care for ill loved ones, and avoid infectious disease spread, all while securing their incomes and their jobs. The United States does not offer its workers a permanent, federally-spanning paid sick leave law. Instead, it is typically the purview of employers to provide their workers with paid time off or sick leave benefits. To promote broader sick leave coverage, some states, counties, and cities have passed mandates which explicitly require employers in their jurisdictions to provide their workers with paid sick leave. Despite these avenues for income and job support during short-term sickness, nearly a quarter of United States workers lack access to paid sick leave.¹

Because a worker without paid sick leave is not guaranteed payment or job security in times of personal or familial illness, she may choose to attend work, even if she is running a fever or caring for a sick child. Today, the global pandemic caused by COVID-19 makes clear: personal decisions to attend work or gatherings while ill or following infection exposure have profound health effects on others. These decisions can expose other communities to infection and may contribute to the spread of infectious disease.

This study focuses on the early months of the COVID-19 pandemic in the United States and investigates if paid sick leave access affected personal choices about whether to stay home or go out. This study also examines if these choices translated into population-level COVID-19 health outcomes such as sickness and death. The early months of the pandemic are critical to study because when it comes to a highly contagious, exponentially growing illness such as COVID-19, proactive mitigation efforts draw the line between an outbreak and an epidemic.² If

¹ DeSilver, "As Coronavirus Spreads"

² Du et al., "Effects of proactive social distancing"; Wang et al., "Response to COVID-19 in Taiwan," 1341-1342

paid sick leave laws were effective at keeping people at home during the start of the pandemic, they may be viewed as valuable measures for preventing widespread infection spread. I center the U.S. because the variation in paid sick leave mandate legislation across different geographic regions gives me the opportunity to examine how at-home behavior, COVID-19 cases, and COVID-19 deaths differed by PSL access.

In order to investigate how PSL access impacted the virus' early course, I compiled a panel dataset ranging from January 2019 to September 2020 which contains high-frequency cell phone location data measuring "stay-at-home behavior" by granular geographic units called census block groups (CBGs); COVID-19 cases and deaths by county; state-level dates for key policy events (e.g. date of first stay-at-home order in state, date of school closures in state); demographic information by geographic region; and state-level indicators for states which were covered by paid sick leave mandates in January 2020. These data provide me with a detailed picture of distancing, COVID-19 cases, and COVID-19 deaths experienced by nearly all census block groups in the United States, where CBGs are categorized by whether or not they were in a state with a PSL mandate. The variation around which CBGs were covered by state-level PSL is key to my empirical strategy because it allows me to create "treatment" and "control" groups and more precisely estimate the effect of paid sick leave access on distancing, cases, and deaths. The data on policy events and demographic shares by geographic region allow me to control for a variety of other factors, such inherent characteristics within a geographic area, that may drive observed outcomes.

Using a difference-in-differences-style model which leverages cross-sectional variation in state-level paid sick leave mandates across the U.S, I find that people in all states responded to informational and policy events regarding COVID-19 by staying at home more. Stay-at-home

behavior following these events was generally more pronounced for people in states with PSL. In precise numbers, I estimate that people covered by state-level paid sick leave mandates stayed at home about 22 more minutes following the stay-at-home orders in their states, relative to people in states not covered by sick leave following their own states' stay-at-home orders. However, additional analysis suggests that the first significant divergence in PSL and non-PSL states' at-home behavior during the pandemic occurred much earlier than state stay-at-home orders. Event studies around behavior-influencing events which occurred from January through the end of March (e.g. first case in country, public health emergency declaration) lead me to a new event of interest when PSL states and non-PSL states were initially on similar trends and then the states' at-home behavior significantly diverged: President Donald Trump's National Emergency Declaration on March 13th, 2020. I estimate that, following this date, people in states with paid sick leave mandates stayed at home about 38 more minutes relative to people in states without paid sick leave mandates. A Regression Discontinuity Design (RDD) around the Declaration event estimates that people in states with PSL exhibited immediate distancing following this event amounting to about 30 more minutes at home per day relative to those without state-level PSL coverage. To put this range of numbers in context, 30-38 additional minutes at home is similar to going from typical at-home behavior on a Friday to typical at-home behavior on a Thursday. Using RDDs with cases and deaths as outcome variables, I find that PSL states had fewer 5-day-lead cases and fewer 14-day-lead deaths after this significant increase in distancing, though the differences in these case and death outcomes were not statistically different from zero.

I also test for heterogeneous effects and find that in PSL states, higher shares of Hispanic people in a CBG significantly decreased the level of distancing which people exhibited--by about

49 minutes for each one percentage point increase in the share of Hispanic people-- while higher shares of college-educated people in a CBG significantly increased the level of distancing which people exhibited-- by about 36 minutes for each one percentage point increase in the share of college-educated people. These results suggest that individual characteristics such as educational attainment and ethnicity were associated with differing capacities to stay home during the pandemic, even when paid sick leave mandates were on the table. Finally, in contrast to these results which look at policy events prior to April 2020 and on-the-books PSL variation, I apply a modified, “reverse” difference-in-differences approach in order to look at the effect of a federal, temporary PSL policy on places which were not already covered by sick leave mandates. This approach addresses whether broadened access to PSL during the pandemic had consequential impacts on distancing, particularly for those who did not previously access paid sick leave through state legislation. My results, while not entirely conclusive due to endogeneity concerns, suggest that the introduction of a federal sick leave policy had a greater effect on distancing among those without previous access to sick leave through their state. Following the FFCRA, people without previous access to paid sick leave through their states stayed at home for about 23 more minutes per day relative to those with previous PSL coverage.

Overall, the estimated effects of PSL mandates on distancing, cases, and deaths are somewhat small in magnitude. The difference in distancing between PSL states and non-PSL states following the National Emergency Declaration was similar to the average difference in distancing between PSL states and non-PSL states throughout the entire year of 2019. Possible explanations for the magnitude of these results could be a busy external context with a number of pandemic-related, behavior-influencing events “competing” with the effect of PSLs, as well as

coverage gaps or law inadequacies of state-level PSLs. I do not empirically explore the effect of “skimpy” mandates in this paper, though future works should.

This thesis most directly contributes to a small body of work which examines the effect of paid sick leave on COVID-19 outcomes such as distancing, cases, and deaths.³ Existing studies find that increased access to sick leave due to the FFCRA resulted in greater distancing and fewer cases and deaths in the United States. However, unlike existing works, this thesis takes a unique approach by focusing on implications of state-level sick leave policies which were already on-the-books at the start of the pandemic. More broadly, this thesis engages with COVID-19 era literature focused on social distancing.⁴ Social scientists during COVID-19 have been in pursuit of greater clarity on the determinants and consequences of social distancing, as well as distancing’s potential causal pathway away from virus spread. Researchers have studied policy events such as shelter-in-place orders and non-essential business closures, and focused their attention on how community characteristics such as income level and partisanship impact compliance with distancing mandates and advisories. State-level paid sick leave mandates can be viewed as a community characteristic which may have influenced distancing, and possibly the course of the virus. Finally, although this work is well situated within the Coronavirus paradigm, its focus on the public health implications of paid sick leave places it among sick leave studies that focus on the U.S. and the rest of the world.⁵

³ Andersen et al., “Effect of a federal paid sick leave mandate”; Pichler, Stefan, Katherine Wen, and Nicolas R. Ziebarth, “COVID-19 Emergency Sick Leave Has Helped Flatten,” 2197-2204

⁴ Friedson et al., “Did California’s Shelter-in-Place Work?”; Dave et al., “When do shelter-in-place orders fight COVID-19 best,” 29-52; Abouk, Rahi, and Babak Heydari, “The immediate effect of COVID-19 policies,” 245-252; Gupta et al., “Tracking public and private responses to the COVID-19 epidemic”; Courtemanche et al., “Strong Social Distancing Measures,” 1237-1246; Fowler et al., “The effect of stay-at-home”; Wright et al., “Poverty and economic dislocation,” 544-554

⁵ Maclean et al. “Mandated sick pay”; Callison, Kevin, and Michael Pesko, “The effect of paid sick leave,” 16-25; Stearns, Jenna, and Corey White. “Can paid sick leave mandates reduce leave-taking?,” 227-246; Markussen, Simen. “The individual cost of sick leave,” 1287-1306

The remainder of this paper is organized as follows. Section II contains additional context on the COVID-19 pandemic in the United States and situates this study at the intersection of works focused on pandemic-era public health interventions and works on paid sick leave. Section III discusses the key data sources that I will be using in order to perform my analysis, with attention to their potential limitations. In Section IV, I present my empirical strategy, results, and analysis. Section V concludes.

II. Background & Literature Review

A. COVID-19 Overview

In late 2019, a cluster of severe respiratory illnesses was first reported in Wuhan, the capital city of China's Hubei province.⁶ As an increasing stream of patients with similar symptoms checked into Hubei-area health centers, the World Health Organization (WHO) took notice.⁷ By February 2020, the virus linked to the reported illnesses had a name-- severe acute respiratory syndrome coronavirus 2 (SARS-CoV-2)-- and the illness became widely known as Coronavirus disease 2019, abbreviated COVID-19. On March 11th 2020, after 118,000 cases and 4,291 deaths were detected across 114 countries, the WHO declared the Coronavirus disease 2019 a worldwide pandemic.⁸ As of April 2021, the WHO has reported over 133 million worldwide COVID-19 cases and nearly 3 million worldwide COVID-19 deaths.⁹

Despite containing 4.25% of the world population, the United States is responsible for approximately 20% of total worldwide WHO-reported Coronavirus cases and deaths as of April 2021.¹⁰ The illness' spread and impact in the U.S. reflects deeply-rooted inequities. For example, individuals who are already at the margins due to racism and disparities in healthcare resources,

⁶ Huang et al., "Clinical features," 497-506

⁷ World Health Organization, "Pneumonia of unknown cause"

⁸ World Health Organization, "WHO Director-General's opening remarks"

⁹ World Health Organization, "WHO Coronavirus Disease (COVID-19) Dashboard"

¹⁰ Ibid

income, and wealth access have comprised a disproportionate number of U.S. COVID-19 cases and deaths.¹¹ This thesis closely examines public health measures and community behavior in the United States during early 2020 in order to glean vital information about how the pandemic reached its current form.

B. Worldwide Response to COVID-19

Since the Coronavirus pandemic's start, health experts have pointed to a slew of policies which help minimize the virus' spread. Mobility restrictions have been some of the earliest and longest-sustaining interventions. On January 23rd, 2020, China enforced the first set of city and province-level travel restrictions meant to contain the then-little-known Coronavirus by minimizing the virus' community transmission. China's lockdowns in Wuhan and other Hubei province cities were the largest in public health history at the time of their enforcement.¹² Initially viewed as controversial due to their scope and unprecedented nature, there is evidence that the lockdowns succeeded in reducing human mobility and virus spread: an early study utilizing difference-in-differences estimation strategies and smartphone mobility data found that the Wuhan lockdown reduced city inflow, outflow and within-city movement by 76.64%, 56.35%, and 54.15%, respectively, and reduced projected cases within the Hubei province by 52.64%.¹³ According to the authors' results, the counterfactual to the lockdown—a world where Wuhan did not experience a city-wide lockdown on January 23rd, 2020-- would likely have resulted in 105.27% more fatalities in 347 Chinese cities outside the Hubei region. Additionally, although cultural, political, and economic differences made the precise mechanics of China's lockdowns difficult to replicate in other worldwide settings, the lockdowns' empirically-tested effects on population movement and virus spread created a strong case for human mobility

¹¹ Chowkwanyun, Merlin, and Adolph L. Reed Jr, "Racial health disparities," 201-203

¹² Fang et al., "Human mobility restrictions"

¹³ Ibid

restrictions as a useful sickness mitigation strategy. As such, by March 2020, over 100 countries around the world followed China's lead and instituted either partial or full lockdown orders intended to suspend all non-essential contact between population members.¹⁴ These policies often led to reductions in case counts and deaths in jurisdictions where they were enforced, relative to their counterfactual scenarios.¹⁵

C. United States Response to COVID-19

Despite worldwide precedent for nationwide lockdown orders as virus spread minimizers, the United States federal government never enacted policies which explicitly required individuals to stay at home. Rather, beginning in March 2020, local authorities within states, counties, and cities instituted their own pandemic policy responses, resulting in a region-specific patchwork of interventions and non-interventions ranging in duration and scope.¹⁶ Some jurisdictions enacted stay-at-home orders, while others instituted a mix of stay-at-home orders, school closures, and non-essential business closures.¹⁷ A few states did not enforce any policies which required people to stay home.¹⁸

On the U.S federal government side, actions from January 2020 through March 2020 taken in response to the pandemic included a Public Health Emergency Declaration by the Health and Human Services Secretary on January 31st, 2020 which gave local health authorities more flexibility to assign personnel to pandemic response tasks; travel bans proclaimed by President Donald Trump from places including China, the UK, and the EU; a National Emergency Declaration delivered by President Trump on March 13th, 2020 which freed up disaster and emergency relief resources for pandemic relief under the Stafford Act; general

¹⁴ British Broadcasting Corporation, "Coronavirus"

¹⁵ Alfano, Vincenzo, and Salvatore Ercolano, "The efficacy of lockdown," 509-517

¹⁶ Kates, Jennifer, Josh Michaud, and Jennifer Tolbert, "Stay-At-Home Orders"

¹⁷ Kaiser Family Foundation, "State COVID-19"

¹⁸ Ibid

advisories by President Trump encouraging social distancing beginning on March 16th, 2020; and a 1.8 trillion dollar relief package designed and passed by Congress and the White House on March 27th, 2020 to minimize the economic and social fallout associated with the pandemic.¹⁹ On April 1st, 2020, the country's first federal, temporary paid sick leave policy via the Family First Coronavirus Response Act went into effect. Designed by Congress and effective until the end of 2020, the FFCRA enabled the federal government to reimburse certain public employers and private employers with fewer than 500 employees for up to two weeks of paid leave which their workers took.²⁰ Valid reasons that workers could be exempt from work included COVID-19-related emergencies such as sickness, necessary isolation, and childcare.²¹ Despite the unprecedented nature of the Act, it faced some limitations in terms of timing and scope. By the time the FFCRA's paid sick leave provision went into effect, community transmission in the United States was in motion: on April 1st, 2020, over 200,000 cumulative COVID-19 cases had already been reported by U.S. public health agencies to the WHO.²² Due to eligibility terms, the emergency paid sick leave provided by the FFCRA left out an estimated 87 million workers, including workers in essential workplaces such as pharmacies and grocery stores who were often required to come to work during the pandemic.²³

D. Social Distancing in the U.S: A Key Virus Mitigation Strategy

From state-level stay-at-home orders to the FFCRA, The United States' broad focus on social distancing mirrors worldwide pandemic responses. Researchers examining fluctuating

¹⁹ Ballotpedia, "Federal government responses"

²⁰ Congress.gov, "H.R.6201"

²¹ Ibid

²² World Health Organization, "WHO Coronavirus Disease (COVID-19) Dashboard"; The United States' true case and death numbers are likely even higher than the WHO's reported counts due to undercounting, inadequate testing, and reporting errors, especially at the pandemic's outset (Basu, Anirban, "Estimating the Infection Fatality," 1229-1236; Brown et al., *The Washington Post*, April 27, 2020).

²³ Fowers, Alyssa and Shelly Tan, *The Washington Post*, March 19, 2020; Glynn, Sara Jane, "Coronavirus Paid Leave Exemptions Exclude"

social distancing measures around the U.S have evaluated these measures' causal impacts on human mobility, case counts, and fatalities, often employing smartphone location-tracking data for mobility measurements and COVID-19 infection trackers for case counts and deaths. The general consensus is that social distancing worked-- measures which kept people away from close contact with people outside their household typically succeeded in pushing down case counts and deaths in places where they were enacted.²⁴ However, social distancing measures can take several different forms. Depending on their precise design and other characteristics about the community where they were put in place, measures impacted individuals differentially. Thus, variation in social distancing literature occurs as researchers attempt to parse through this noise. Still, the question of “what circumstances allow for social distancing and the associated reduction of cases and deaths to occur most optimally” remains a common thread through existing literature and motivates this particular paper.

Studies have found that the specific type of policy mattered for determining the extent of distancing and thus the virus' mitigation, though they conflict in their assessments of what the optimal policy measure was. A study of six interventions in the United States—bans on large gatherings, school closure mandates, statewide stay-at-home, limited stay-at-home orders, non-essential business closures, and restaurant and bar limits—employed Google Mobility Trend Data and case count data from the COVID Tracking Project and found that stay-at-home orders were most effective in reducing social contact and confirmed cases, while other policies such as non-essential business closures and bar limits led to smaller, statistically insignificant effects on mobility and cases.²⁵ Meanwhile, a study which employed human mobility data from SafeGraph

²⁴ Friedson et al., “Did California’s Shelter-in-Place Work?”; Dave et al., “When do shelter-in-place orders fight COVID-19 best,” 29-52; Abouk, Rahi, and Babak Heydari, “The immediate effect of COVID-19 policies,” 245-252; Gupta et al., “Tracking public and private responses to the COVID-19 epidemic”; Courtemanche et al., “Strong Social Distancing Measures,” 1237-1246; Fowler et al., “The effect of stay-at-home”; Wright et al., “Poverty and economic dislocation,” 544-554

²⁵ Abouk, Rahi, and Babak Heydari, “The immediate effect of COVID-19 policies,” 245-252

and PlaceIQ found that shelter-in-place orders did not substantially increase at-home behavior, rather informational events such as news about the first case in a county or partial closure policies such as business or school closures had a larger effect on keeping individuals at home.²⁶ Another work which estimated the effectiveness of large gathering bans, school closures, non-essential business closures, and shelter-in-place orders in averting COVID-19 cases found that shelter-in-place orders and non-essential business closures were the most effective at reducing the COVID-19's daily case growth, though the authors cited a lack of evidence for making any conclusive claims on the effects of school closures or gathering bans on reducing case counts.²⁷

Despite the limited consensus around the precise measures which caused the most significant change in distancing, cases, and death, the past works produce a long list of policy and informational events which were potential behavior-changers. This list helps to inform my empirical approach since it offers possible events around which stay-at-home behavior exhibited by people in PSL states and non-PSL states possibly diverged. While I performed analyses on nearly all of these events, I only show the results from two of them in Section IV: state stay-at-home orders and the National Emergency Declaration on March 13th, 2020. The state stay-at-home orders were useful empirical events because their asynchronous passages created variation over time of places covered by and not covered by stay-at-home orders. This made for a rich set of comparison groups to conduct analyses on. However, because stay-at-home orders were often passed in response to rising infection rates within states, policy endogeneity may prevent my results from capturing causal relationships between orders and outcomes. As a result, I turn to the National Emergency Declaration on March 13th, 2020, an exogenous event which I

²⁶ Gupta et al., "Tracking public and private responses to the COVID-19 epidemic"

²⁷ Courtemanche et al., "Strong Social Distancing Measures," 1237-1246

empirically find to coincide with a significant divergence in distancing between PSL and non-PSL states during the pandemic.

Beyond type of policy, the characteristics of people where a policy was enacted were crucial determinants of the policy's effectiveness. Studies that segmented the population by characteristics such as income, partisanship, and community trust found that population heterogeneity across geographic areas influenced stay-at-home compliance. In particular, higher-income communities were more likely to comply with lockdown policies than lower-income ones.²⁸ Individuals in predominantly Democratic counties were also more likely to completely stay at home following a stay-at-home order relative to those living in majority-Republican counties.²⁹ A county with more "community trust," as determined by a measure from the General Social Survey, was more likely self-isolate following a stay-at-home order.³⁰ Given the numerous studies which find a causal link between social distancing measures and the virus' spread, the places exhibiting less social distancing and stay-at-home compliance likely had higher infection rates relative to the ones that complied to stay-at-home orders more, though few of the authors mentioned commented on infection-related outcomes. My study is most similar to these works which focus on spatial characteristics; I examine how on-the-books, state-level paid sick leave mandates in the United States affected social distancing. I also examine outcomes such as case counts and deaths in order to see if the virus' course varied between places with and without paid sick leave mandates.

E. Paid Sick Leave Overview

²⁸ Wright et al., "Poverty and economic dislocation," 544-554; Jung et al., "Coronavirus infections," 311-330

²⁹ Painter, Marcus and Qiu, Tian, "Political beliefs affect compliance"

³⁰ Brodeur, Abel and Grigoryeva, Idaliya and Kattan, Lamis, "Stay-at-Home Orders"

A key way to minimize the spread of infectious diseases is to encourage workers to stay home from work if they are feeling sick or if they have been exposed to illness.³¹ The link between sick workers attending work in-person and illness spread exists because workplace presenteeism poses a negative public health externality: a 2010 study on workplace transmission of the A(H1N1)pdm09 virus found that eight million people attended work while sick with the virus, leading to an estimated seven million infected coworkers.³² Works exploring presenteeism outside of the H1N1 context offer additional support for the connection between coming to work while sick and infection spread.³³ By securing workers' jobs and incomes if they need to stay home from work due to personal or familial illness, PSL access has historically been an important determinant of sick and sickness-exposed workers' decisions to stay home.³⁴

Seventy-three percent of United Nation member states guarantee their formal workers some form of paid sick leave beginning from workers' first signs of illness.³⁵ The United States is not one of these countries. Though the 1993 Family and Medical Leave Act allows for 56% of US workers to be eligible for 12 workweeks of yearly job-protected leave, geared specifically for circumstances such as serious health conditions of family members or newborn children, the Act does not guarantee that workers will receive pay for days they miss.³⁶ While there currently exists no permanent, federal paid sick leave law in the United States which ensures that sick workers will be compensated when they need to skip a day of work, some employers voluntarily provide their employees with leave pay. However, this employer-focused scheme for leave leads

³¹ Center for Disease Control and Prevention, "CDC Recommendations"; Center for Disease Control and Prevention, "Preventing the Spread"

³² Drago, Robert and Kevin Miller, "Sick at work"

³³ Webster et al. "A systematic review of infectious illness presenteeism," 1-13; Pichler, Stefan and Nicolas R. Ziebarth. "The pros and cons," 14-33

³⁴ DeRigne et al., "Workers without," 520-527; Piper et al., "Paid sick days and stay-at-home behavior for influenza"; Kumar et al., "The impact of workplace policies," 134-140

³⁵ Heymann et al, "Protecting health"

³⁶ Brown et al., *Employee and worksite perspectives*

to differences in coverage, particularly for part-time workers and workers in the service industry.³⁷ These differences in coverage by industry, occupation, and firm type lead to gaps in coverage that differ by demographic group. For example, 92% of workers who belong to the top quarter of earners do receive some form of sick leave from their employers.³⁸ This share is just 51% in the lowest quarter of earners and 31% in the lowest tenth of earners.³⁹ Hispanic people are less likely to have access to paid sick leave, in part because they are more likely to work in the service sector and in smaller firms, where coverage is low.⁴⁰

In the absence of a federal sick leave policy, local jurisdictions have passed sick pay legislation for their workers: in January 2020, 12 states, 3 counties, and 17 cities in the United States had either a paid sick leave mandate or a paid time off law in effect. While each of these laws compel employers to cover workers' pay when they are at home tending to their own illness or caring for a sick loved one, they are not all uniform. I will use the January 2020 version of the Massachusetts Earned Sick Time Law in order to illustrate one potential PSL law design.⁴¹

In Massachusetts, most employers are required to provide their employees with earned sick time after the employee has been employed for at least 90 days. However, employers who employ 10 or fewer workers are not required to pay their workers for time off. United States government employees, certain college-aged and school-aged students, and adult clients involved in educational or vocational training in a Massachusetts licensed program are exempt from the law. Workers covered by the mandate in Massachusetts “earn” 1 hour of sick pay for every 30 hours worked, at a 100% wage replacement rate. Workers may apply this sick pay to time spent at home for their own illness, as well as the sickness of a loved one. At an employer's discretion,

³⁷ United States Bureau of Labor Statistics, “Selected paid leave benefits”

³⁸ DeSilver, “As Coronavirus Spreads”

³⁹ Ibid

⁴⁰ Kumar et al., “The impact of workplace policies,” 134-140; Bartel et al., “Racial and ethnic disparities”

⁴¹ Mass.gov, “Earned Sick Time.”

some workers may use sick time before it has been accrued for a year. Paid hours are capped at 40 per year, though additional earned hours may be “carried over” from one year to the next. Under Massachusetts law, if a worker misses more than three consecutive workdays, an employer may ask for proof of sickness such as a doctor’s note. A public health emergency does not alter the terms of the law.

In Section III.E, I discuss limitations of state paid sick leave laws like Massachusetts’ Earned Sick Time Law.

F. Paid Sick Leave Outcomes

Studies focused on paid sick leave mandates in the United States have used their enactments as natural experiments and have utilized difference-in-differences strategies in order to look for changes in outcomes such as paid sick leave coverage, illness rates, presenteeism, absenteeism, and employment between the periods before and after mandates’ effective dates. For example, studies found that workers in jurisdictions covered by paid sick leave mandates generally experienced greater access to sick leave—with one work finding a 13 percentage point increase in sick leave coverage relative to the baseline in the four years following a state mandate’s passage—and demonstrating pronounced coverage effects for women, white non-Hispanics, and those working in industries with otherwise low paid sick leave utilization.⁴² By covering groups who otherwise lack access to PSL, these mandates ensured that more people had the option to stay at home if they or a household member was feeling unwell.

Beyond access, United States paid sick leave mandates have been found to impact work absences in two directions. Among new workers or workers most likely to gain coverage from laws, paid sick leave mandates increased sick days taken.⁴³ According to a different study,

⁴² Maclean et al. “Mandated sick pay”; Callison, Kevin, and Michael Pesko, “The effect of paid sick leave,” 16-25

⁴³ Ibid

aggregate absences attributed to illness decreased for workers who were directly impacted by the mandates as well as those who were not directly affected, a finding which suggests that when workers obtain paid sick leave, they are less likely to attend work while they are sick, infect their coworkers, increase infection rates on an aggregate level, and require their coworkers to stay at home from work.⁴⁴ Despite the two directions of absences, paid sick leave mandates are linked to significant, improved illness outcomes: as workers increased their at-home behavior when they were ill due to the existence of paid sick leave mandates, influenza-like-illness transmission pneumonia and influenza rates reduced at a state population level.⁴⁵

While studies examining the effect of paid sick leave mandates in the United States found significant evidence that sick leave mandates compelled sick workers to stay home and reduce workplace illness, many were written in the pre-pandemic era about newly-passed mandates. As a result, they cannot be used to disentangle the effects of sick leave during the Coronavirus pandemic. Filling the void of COVID-19-era sick leave literature, a few recent studies have focused on sick leave during the pandemic by using the FFCRA effective date as a natural experiment. One working paper utilized difference-in-differences methods and leveraged the spectrum of FFCRA eligibility (via “non-essential worker” status) across United States counties in order to quantify the temporary law’s effect on distancing.⁴⁶ Using cellphone mobility data, the authors found a short-run decrease in in-person work and a corresponding increase in at-home presence following the FFCRA’s effective date, with effects of the law more pronounced for counties with higher shares of workers who were eligible for law coverage.⁴⁷ Using a difference-in-differences strategy, another study found that states where employees gained access

⁴⁴ Stearns, Jenna, and Corey White, "Can paid sick leave mandates reduce leave-taking?," 227-246

⁴⁵ Pichler, Stefan, Katherine Wen, and Nicolas R. Ziebarth, “Positive health externalities”

⁴⁶ Andersen et al., “Effect of a federal paid sick leave mandate”

⁴⁷ Ibid

to paid sick leave through the FFCRA experienced a statistically significant decrease in approximately 400 fewer new confirmed cases per day.⁴⁸ Crucially, neither of these studies examined the effect of pre-FFCRA paid sick leave laws on population mobility, case counts, and deaths. Preliminary data on leave take-up from national and international places with on-the-books, sickness-related income support laws suggest that these early months warrant a closer look.

According to available data from OECD countries such as Austria, Chile, Finland, and Germany, as COVID-19 case counts grew in each country, sick leave claims increased by between 30% and 50% relative to rates from baseline levels measured a year earlier.⁴⁹ These sick leave take-up trends occurred alongside a fall in new COVID-19 infection rates within these countries, suggesting that the existence of paid sick leave laws may have contributed towards a reduction in COVID-19's early spread by allowing workers to stay home while they were sick.⁵⁰ In the United States, there are no current studies on the effect of existing PSLs on outcomes such as mobility, infection, and death. However, state-level claims for temporary disability insurance and family caregiving leave in California and Rhode Island surged in March 2020, indicating an

⁴⁸ Pichler, Stefan, Katherine Wen, and Nicolas R. Ziebarth, "COVID-19 Emergency Sick Leave Has Helped Flatten," 2197-2204

⁴⁹ Organisation for Economic Co-operation and Development, "Paid sick leave to protect income, health, and jobs"

⁵⁰ Ibid; The potential relationship between international sick leave take-up and infection declines must be tempered by a few words of caution. First, some countries with generous sick leave programs (e.g. Sweden) still became COVID-19 hotspots. Contradictions between generous sick leave regimes and infection spread should remind readers that while international safety nets appear generous when compared to the United States, they may not have been sufficient to halt COVID-19's spread. Taking Sweden as one example of a place with a more generous paid sick leave program, the country also employed a "herd immunity" approach to the virus with few explicit restrictions on population movement or gathering. Sweden's laissez-faire pandemic stance possibly curtailed the efficacies of paid sick leave policies as people acted in accordance to the government's signals and continued to expose themselves to illness. Additionally, PSL is just one aspect of countries' responses to the pandemic. Countries used a variety of measures, such as job retention schemes which paid firms to keep their employees on payroll even if they were not working, to make sure sick people self-isolated. Policies such as these may have increased the level of distancing exhibited by people in these countries. Without a rigorous methodological approach, it is difficult to determine how paid sick leave operated in these countries during the early months of the pandemic while leaving out the effects of other policies and government approaches. In my study, I am able to move beyond preliminary data by leveraging the variation in PSL across the United States in order to rigorously test for the effect of paid sick leave in the U.S.

increase in need for leave-related income support at the start of the pandemic.⁵¹ Worldwide and U.S trends motivate a look into the period prior to the FFCRA’s passage in order to see how already on-the-books paid sick leave laws may have affected COVID-19’s spread in the United States. Unlike the countries with nationwide PSL laws, the United States is a unique case which contains geographic variation of PSL laws. This variation allows me to compare outcomes in places with PSL mandates with their counterfactual, no-PSL scenarios. By investigating the impact of state-mandated PSL policies on COVID-19-era worker outcomes during the period prior to the FFCRA’s enactment in the United States, this work approaches a novel area of inquiry.

III. Data

A. Social Distancing

In order to capture United States social distancing over time within relatively granular geographic areas, I utilize high-frequency, anonymized cellular device location data known as Social Distancing Metrics (v2) offered by SafeGraph Inc., a data company.⁵² Each day, SafeGraph collects a random sample of GPS “pings” from cellular devices and calculates 15 measures related to mobility using these devices’ locations. To protect device owners’ privacy, SafeGraph aggregates devices within a census block group (CBG)--a Census Bureau-designated geographic unit which contains between 600 and 3,000 people-- and removes CBGs which contain fewer than 5 unique devices. For my analysis, I extract CBG observations from January 1st, 2019-September 26th, 2020. Since the Social Distancing Metrics collects mobility data each day across more than 200,000 CBGs, this extract alone contains over 140 million observations. Computing constraints require me to restrict my analyses to smaller subsamples of these data.

⁵¹ Boyens, Chantel, “State Paid Family and Medical Leave Programs’

⁵² SafeGraph data available at <https://www.safegraph.com/>.

From the Social Distancing Metrics, I utilize the “median home dwell time” variable which calculates the median number of minutes per day in which devices in a CBG spend at home.⁵³ I use this measure because it is commonly employed by other COVID-19 studies as a proxy for social distancing and it can be interpreted with relative ease.⁵⁴ Throughout the course of my analysis, I tried a number of other distancing measures from the SafeGraph data and found the results to be qualitatively similar to the ones that use median home dwell time as the outcome.⁵⁵ I chose not to measure median home dwell time in logs because the distribution of the raw median home dwell time measures is approximately normal. To assist with interpretation, I could alternatively show the daily minutes at home as a share of 1440, the total number of minutes in a day. However, a comparison between measured minutes and median home dwell time across different days of week and months in a more “typical” year serves a similar purpose. As shown in Table 1, the average value of the median home dwell time measures for CBGs in PSL states and non-PSL states in 2019 was about 666 minutes and 635 minutes, respectively. Table 2 shows that in a typical week in 2019, people in all states stayed at home the most on Sundays and the least on Fridays. The average at-home behavior in all CBGs by month of year is presented on Table 3: people stayed at home the most in November and the least in May.

Figure 1 shows how the median home dwell time measure varied in CBGs from January 1st, 2020 through May 1st, 2020, disaggregated by whether or not a CBG was in a state covered by a PSL mandate. I include demarcations for federal COVID-19 policy and informational events which the country underwent. Figure 1 suggests that in January 2020, distancing behavior

⁵³ Home locations for devices are calculated using an algorithm which searches for a common nighttime (6pm-7am) location across a 6-week period for each device at a Geohash-7 precision.

⁵⁴ Examples of other studies which use this measure: Farboodi et al., “Internal and external effects of social distancing”; Andersen et al., “Effect of a federal paid sick leave mandate”; Yan et al., “Measuring voluntary and policy-induced social distancing”

⁵⁵ Other tested measures: median percentage home dwell time, mean home dwell time, mean percentage home dwell time, share of devices home for the full day, share of devices home for more than 75% of the day, share of devices home for more than 50% of the day, share of devices home for more than 25% of the day.

among people in PSL and non-PSL states fluctuated, though the average difference in at-home behavior between the two kinds of places stayed relatively constant. At this time, people who were not covered by PSL mandates stayed at home more relative to people who were covered by mandates.⁵⁶ Following the first reported case in the United States on January 21st, 2020, people in all states began to leave their homes *more* and the distancing “gap” between people in PSL states and people in non-PSL states closed. At the end of February, stay-at-home behavior exhibited by people in all states rose, though a growing difference in at-home behavior between people in PSL states and people in non-PSL states became visible in mid-March soon after President Trump’s National Emergency Declaration on March 13th, 2020. Right around the first stay-at-home order in California on March 19th, 2020, people in all states exhibited a stark drop in at-home behavior. This drop may reflect a national rush to stock up on essential groceries and other goods soon after a stay-at-home order in a state or in anticipation of an order to come.⁵⁷ This drop rebounded in late-March, and by the time the FFCRA effective date rolled around on April 1st, 2020, people in all states exhibited greater at-home behavior than they did in January. Similar to the distancing gap in the immediate, post National Emergency Declaration period, people in PSL states seemed to stay at home much more than people in non-PSL states throughout the month of April.

Despite the widespread reliance on SafeGraph Social Distancing Metrics and the particular median home dwell time measure throughout pandemic-era distancing literature, the data may suffer from some limitations. First, since the data proxy population movement using cellular device movement, they do not account for individuals who do not own cellular devices,

⁵⁶ Evidence that people in non-PSL states were at home more (and not at work) in January may refute the idea that PSL laws lead to more workplace absenteeism.

⁵⁷ This behavior shows that stay-at-home orders may have had unintended consequences on sickness spread in their immediate aftermath, as people left their homes more (and exposed themselves to sickness) in an effort to stock up on toilet paper and milk for the days ahead.

who do not carry cellular devices, or who switch location services off. Other problems are more technical, related to data sampling: changing composition of daily devices due to random sampling, fluctuations in numbers of devices from a CBG that are sampled each day, sampling bias towards moving devices, and other general errors that come along with tracking GPS signals. There may also be issues with the calculation of a “home location.” The home location will be incorrect for individuals who do not consistently spend nighttime hours at their homes (e.g. night-shift workers). I do not expect any of these issues to significantly bias my results because I assume that they apply to cellular devices in all jurisdictions and do not systematically apply differently to people in places with PSL relative to those without PSL.

B. Cases and Deaths

The COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts are my primary sources for COVID-19 case and death data. The JHU data have been trusted for real-time, worldwide case and death counts since their initial release on January 22nd, 2020, one day after the first reported case in the country.⁵⁸ Meanwhile, USAFacts is a nonprofit, non-partisan organization committed to making government data accessible for civic use.⁵⁹ The organization’s COVID-19 tracker launched in March 2020.

Both USAFacts and JHU collect health data directly from state and local health agencies and offer county-level, daily case and death panels. The county-level precision and daily frequency of these two sources allow me to perform a more nuanced analysis than analyses performed using other commonly-cited sources which report state or country-level cases and

⁵⁸ Data available at <https://github.com/CSSEGISandData/COVID-19>.

⁵⁹ Data available at <https://usafacts.org/visualizations/coronavirus-covid-19-spread-map/>

deaths. The JHU data comprises the bulk of my data on cases and deaths, though I use the USAFacts data in order to correct for oddities in the JHU data.⁶⁰

According to the combined data, the first COVID-19 case in the United States was reported on January 22nd, 2020 in Washington State, a state with a state-level PSL mandate. This case report is lagged 1 day from January 21st, the day that the CDC sent out a press release regarding the first U.S COVID-19 case.⁶¹ The data reports that the first case in a state without PSL was Illinois on January 24th, 2020, one day after news about the case was transmitted to media outlets.

Both data sources likely understate the true magnitude of infection and fatalities in the United States.⁶² Additionally, other factors beyond sickness could be driving differential case or death counts, such as access to testing, hospital system preparation, and the public's trust in public health authorities. Other studies investigating COVID-19 cases and deaths stick to these measures since there exist few alternatives.⁶³

C. Demographic Details

Census block group-level demographic group shares from the 2016 American Community Survey (ACS) 5-year estimates allow me to assess if observed outcome differences between PSL and non-PSL states occurred as a result of demographic characteristics. The 5-year estimates contain rich information about social, economic, and housing characteristics within

⁶⁰ The JHU data combines Dukes and Nantucket Counties in Massachusetts, groups together counties in Utah by region, and reports cases for Kansas City, Missouri rather than cases in the counties that the city lies in. I merge in the corrected county counts from the USAFacts dataset.

⁶¹ Centers for Disease Control and Prevention, "First Travel-related Case"

⁶² Noh, Jungsik, and Gaudenz Danuser, "Estimation of the fraction of COVID-19 infected people in US states and countries worldwide"

⁶³ Some researchers navigate the underreporting of deaths by utilizing a different metric of the COVID-19 pandemic's toll, such as excess mortality. However, at the time of my data collection, excess death mortality data were not available for public use on a county-level over time.

CBGs. In my data, these are fixed characteristics because I only have these measures for one time period.

Table 4 shows that CBGs in PSL and non-PSL states contain qualitatively similar shares of demographic groups, though there exist a few differences in shares which exceed 1 percentage point. CBGs in non-PSL states have larger shares non-Hispanic White people and Black people (9 percentage points more and 6 percentage points more, respectively). Meanwhile, CBGs in PSL states have larger shares of Hispanic people (4 percentage points more), Asian people (6 percentage points more), college graduates (6 percentage points more), households with annual incomes greater than \$200,000 (3 percentage points more), and people who work from home (1 percentage point more). While the ACS demographic shares are dated by 4 years and there certainly was some population movement in the years from 2016 to 2020, I assume that demographic shares have remained relatively stable since 2016.⁶⁴ This assumption allows me to implement CBG fixed effects which control for static heterogeneity across CBGs that could drive the level of distancing which people in that CBG exhibit.

Despite including CBG fixed effects which control for static heterogeneity across geographic areas, shares of demographic groups could interact with the presence of state sick leave mandates in ways that are significant. For example, it is possible that people in PSL states with greater shares of college graduates distanced significantly more than people in PSL states with smaller shares of college graduates. Finding this effect would indicate that there exist other significant factors in addition to PSL which contribute to the level of distancing exhibited by people within a CBG. In order to test for heterogeneous effects, I utilize three measures from these data: “share non-Hispanic Black,” “share Hispanic,” and “share college-educated.” The

⁶⁴ Despite pronounced news coverage on COVID-induced population movement out of cities, recent data show that permanent American movement trends in 2020 were generally not very different from 2019 trends (Kolko et al., The New York Times, April 19th 2021).

first two measures capture information about race and ethnicity, while the third correlates with income and job security and could be a good proxy for these other attributes.

D. Policy Events

For effective dates of state-level policies including stay-at-home orders, non-essential business closures, restaurant closures, school closures, and state-level state of emergency orders, I use the COVID-19 US State Policy Database.⁶⁵ This database is developed and maintained by Boston University School of Public Health Researchers and receives new updates at least twice a week. The researchers source the policy information directly from state government websites in order to make manual additions or corrections to the existing database. Figure 2 shows that places with paid sick leave generally passed earlier stay-at-home orders. Combining data on stay-at-home orders, infections, and fatalities, Figures 3-4 show that stay-at-home orders were typically passed in states once cases and deaths within states were on the rise.

E. Paid Sick Leave Laws

A Better Balance, a national nonprofit advocacy agency which supports policy initiatives such as Paid Medical and Family Leave, fair scheduling, and quality eldercare, is my primary source for information on state-level paid sick leave laws and paid time off laws, which I treat as similar.⁶⁶ The organization compiles a variety of resources which detail eligibility criteria, accrual start dates and rates, and caps on yearly-allotted leave. I cross-checked state government websites for effective dates of these laws.

My analysis only focuses on state-level PSL laws and omits laws passed on a county and on a city level. I do not include city and county level mandates because I worry that these mandates were passed in a more localized policy context, which increases the difficulty of

⁶⁵ Data available at <https://github.com/USCOVIDpolicy/COVID-19-US-State-Policy-Database>

⁶⁶ A Better Balance, “Overview of paid sick time laws”

differentiating the effect of being covered from a sick leave mandate from the effect of being in a jurisdiction where people want policies which offer them income support during times of sickness (and might respond to information in unique ways). This problem may still persist with state-level laws, though I expect for its effect on the results to be to a lesser degree. Table 5 lists all PSL laws in effect as of April 2021.

It is also worth noting that while I treat each mandate uniformly in my analysis, they are not all the same. State-specific terms such as eligibility criteria, accrual mechanisms, and yearly hours “caps” control the level of generosity which a PSL law provides workers. Less generous laws due to employer exemptions, a large hours-worked-to-hours-earned ratio, and restricted hours available per year means that a worker may still suffer from lack of sick time coverage and will still attend work if she feels sick or has been exposed to sickness. Less generous laws might mean that the measured effect of PSL mandates is biased downwards. Across all laws, the maximum number of hours provided by a mandate is 48, which is equivalent to about six work days. Six days do not cover the full two-week quarantine period recommended by public health officials during the COVID-19 pandemic. Additionally, depending on the terms of a law, if a worker was new to a firm in 2020, did not “carry over” hours from a previous year, or was newly covered by a sick leave mandate, she may not have accrued enough sick time by the time of the pandemic in order to stay home. Ultimately, this variation in generosity means that PSL states “in-name-only” may not actually have provided their workers with sufficient coverage in order to stave off the public health effects of COVID-19. Finally, since some laws allow employers to require documentation for prolonged absence periods, a worker might not have been able to use sick time if she stayed home due to fear of contracting infection. I keep these caveats in mind as I proceed with my analysis of these laws’ effects.

IV. Empirical Methods & Results

A. Baseline

1. Method

For my baseline model, I leverage variation in exogenous informational and policy events across states over time, as well as spatial variation in state-level paid sick leave mandate coverage, in order to estimate the impact of paid sick leave access on stay-at-home behavior. Specifically, I estimate the difference in median home dwell time between people in places covered by paid sick leave mandates relative to people in places not covered by paid sick leave mandates, following a behavior-influencing event. After controlling for a variety of factors and testing my assumptions, I may attribute estimated differences in distancing between PSL and non-PSL places to paid sick leave.

The intuition of my baseline model is based on two premises. Premise one: the early months of the pandemic in the United States contained behavior-influencing events which informed individuals of COVID-19's threat. Some of these events encouraged people to follow public health orders and stay home when ill or exposed to illness. For example, President Trump's National Emergency Declaration on March 13th, 2020 was, to the general public, an informational event which called attention to the pandemic. Meanwhile, state stay-at-home orders were policy events explicitly designed to keep people at home. Premise two: a worker's capacity to stay home from work when she is sick or has been exposed to illness is influenced by whether or not she has access to paid sick leave. Combining these two premises, we might expect a worker who became aware of COVID-19's threat through an informational or policy event and wished to stay home due to personal or familial sickness to stay home only if missing out on a day of work did not mean losing out on pay. Since paid sick leave mandates guarantee workers

compensation for a missed day, we may expect people covered by paid sick leave mandates to have exhibited greater distancing behavior relative to people not covered by paid sick leave mandates following an informational or policy event related to the pandemic.

The baseline model utilizes the precise timing of two possible events-- the National Emergency Declaration and state stay-at-home orders-- in order to construct a “pre” and “post” period around days when we expect all people to have been informed about COVID-19 and chosen to stay home more. Note that the National Emergency Declaration occurred on the same day for everyone, while state stay-at-home order dates differed depending on the state one was in. If paid sick leave mandates were effective strategies for keeping people at home, I expect to see people covered by paid sick leave mandates staying at home *more* following these behavior-influencing events, relative to people without paid sick leave mandate coverage. The baseline specification takes the following form:

$$\text{MedianHomeDwellTime}_{gst} = \beta_0 + \beta_1 \text{PostEvent}_{gst} + \beta_2 \text{HasPSL}_{gst} + \beta_3 \text{PostEvent}_{gst} * \text{HasPSL}_{gst} + \alpha_g + \gamma_s + \omega_t + \epsilon_{gst}$$

where $\text{MedianHomeDwellTime}_{gst}$ is the estimated median minutes spent home for an individual within a census block group on day t . This physical mobility measure, which I will refer to as “distancing,” represents the extent to which individuals stayed home each day during the study period. This value ranges from 0 to 1440-- the total number of minutes in a day--with higher values indicating that a person spent a larger portion of a day at home.⁶⁷

HasPSL_{gst} is an indicator variable which is set equal to 1 if a census block group on day t is covered by a state-level paid sick leave mandate. This indicator does not vary for observations

⁶⁷The average value of the median home dwell time measure in 2019 was 641 minutes, which translates to about 10 hours and 41 minutes.

over time within my study period because I restrict my analysis to the early months of 2020 and no new states instituted paid sick leave mandates during this time. $\text{PostEvent}_{\text{gst}}$ is an indicator which is set to 1 if a census block group on day t has already experienced a particular event which encouraged people to stay home. The interaction between $\text{PostEvent}_{\text{gst}}$ and $\text{HasPSL}_{\text{gst}}$ is 1 if a census block group is covered by paid sick leave on a day after the event associated with $\text{PostEvent}_{\text{gst}}$ occurs, and 0 otherwise. α_g are census block group fixed effects which account for variation in distancing attributed to inherent, static characteristics of census block groups. γ_s are county-month of year-day of week fixed effects. I use γ_s in order to control for location-based, monthly, and day-of-week differences in distancing because I expect, for example, a person living in Hennepin County, Minnesota to exhibit different at-home behavior on a Sunday in January relative to a person on a Sunday in January living in Harris County, Texas. When I restrict my study period to 2020, γ_s also control for fluctuating unemployment levels on the county and state level which change month-to-month and could be affecting distancing behavior.⁶⁸ ω_t are day fixed effects which control for the effect of national events that everyone in the study sample experiences, such as President Trump delivering a speech, on distancing. In order to account for correlated error terms across individuals living in the same state, I cluster standard errors by state.

The coefficient β_3 on the interaction between $\text{PostEvent}_{\text{gst}}$ and $\text{HasPSL}_{\text{gst}}$ estimates the expected effect of being in a state covered by a paid sick leave mandate, in addition to the effect of being in the “post period” of either the first stay-at-home order in a state or the National

⁶⁸ Since paid sick leave mandates are tied to employment, unemployment reduces the number of people with access to state-level paid sick leave. If unemployment causes people to be at home more, higher rates of unemployment in states with paid sick leave means that I may expect the interaction between $\text{PostEvent}_{\text{gst}}$ and $\text{HasPSL}_{\text{gst}}$ to be biased upwards. However, if people spend less time at home after losing their jobs, higher unemployment rates in states with paid sick leave would cause a number of people to be at home less, therefore biasing the interaction between $\text{PostEvent}_{\text{gst}}$ and $\text{HasPSL}_{\text{gst}}$ downwards.

Emergency Declaration, on minutes spent at home each day. A positive coefficient means that I expect for people in places covered by paid sick leave to, on average, stay at home more relative to people in places not covered by paid sick leave after a behavior-influencing event has occurred. This would indicate that having paid sick leave coverage is an effective way to keep people at home during a pandemic. On the other hand, if the sign on this coefficient is negative, I expect for people in places with paid sick leave to distance less than people in places without paid sick leave following a particular event. Such a result indicates that having paid sick leave coverage is not an effective means for keeping people at home during a pandemic.

2. Potential Pitfalls

The COVID-19 era is an empirically messy period to investigate because a number of events and interventions which influenced distancing occurred within a short time period. These circumstances increase the challenge of drawing a causal link between paid sick leave coverage and social distancing. I must be mindful of the ways that these factors influence my results.

I have to be sure that the event which I choose for $\text{PostEvent}_{\text{gst}}$ is exogenous and that I observe no significant pre-trends in distancing behavior between PSL and non-PSL states, leading up to this event. This is important for a number of reasons. For one, there could be spillover effects of behavior-influencing events across state lines. Perhaps people living in Washington, a state with paid sick leave which had a stay-at-home order go into effect on March 23rd, 2020, were already responding to the March 19th stay-at-home order in neighboring California. Without accounting for the distancing increase in Washington caused by California's stay-at-home order, I could be overstating the effect of paid sick leave mandates on distancing behavior. Equally important, if state stay-at-home orders were only passed when cases were high in a state, people may already have been voluntarily distancing more prior to their own states'

stay-at-home orders in an effort to avoid infection. Such early distancing would only bias my results if people in places with paid sick leave experienced earlier waves of cases and were responding to the rising public health hazard. This bias does potentially exist since PSL states faced earlier “first cases” in their counties. PSL states also, on average, instituted earlier stay-at-home orders, likely in response to large numbers of cumulative cases in their states.⁶⁹

Finally, school closures or non-essential business closures in PSL states happening at the same time as events which I choose may have resulted in greater distancing in these locales. These trends may affect results for the stay-at-home order event as well as my other event--Trump’s National Emergency Declaration.

In order to account for PSL places simply being on different “distancing paths” leading into my chosen events, I include county linear time trends. As robustness checks, I try out my baseline specifications with lagged COVID-19 cases and deaths to see if adding these variables influences my coefficients. I also try adding in indicators for alternate policy events such as school closures and non-essential business closures. In addition, I conduct event studies to closely examine whether there are differential trends in stay-at-home behavior for PSL and non-PSL states leading up to the states’ stay-at-home orders and the National Emergency Declaration.

So that I may interpret β_3 as the causal effect of having paid sick leave on distancing, I must assume that, absent paid sick leave mandates, there would be no significant differences in average distancing between the states which I currently code as having PSL relative to the ones that are coded as not having PSL, following my chosen event. In order to strengthen the plausibility of this assumption, I include CBG, day, and county-month of year-day of week fixed

⁶⁹ See Figures 2 and 5; California, which passed the nation’s first stay-at-home order on March 19th, 2020 was an outlier to this trend. Their stay-at-home order was somewhat preemptive, considering the size of their population. Prior to March 19, the state had 910 reported cases and 19 reported deaths.

effects. These fixed effects account for other factors related to location, policy environment, season, and day of the week which could be driving the differences in distancing behavior which I observe. In Section IV.C, I also test the possibility that the proportions of people belonging to specific demographic groups across PSL and non-PSL places are driving the observed differences in distancing behavior.

Even after taking all of these measures to address endogeneity, directly attributing the average observed differences between the two kinds of places in the entire “post-period” to paid sick leave could still be leaving out unobserved, time-varying heterogeneity between them. I should be cognizant of remaining unaccounted-for factors as I proceed with my results and interpretation.

3. Results

First Stay-at-Home Order in State

Table 6 presents the results for the effect of paid sick leave mandate coverage on social distancing following the first stay-at-home order within a state. I restrict my study period from January 2020 through the end of April 2020 since I hope to concentrate my analysis on the periods just before and just after the first stay-at-home orders’ effective dates. Column 1 is the specification without any fixed effects. I layer up the subsequent models with census block group fixed effects, date fixed effects, and county-month of year-day of week fixed effects. For columns 2-4, the coefficient on $\text{HasPSL}_{\text{gst}}$ is wiped out by the census block group fixed effects because census block group fixed effects capture whether or not a person is covered by a paid sick leave mandate and there is no variation in whether a state has PSL in early 2020. After including census block group, date, and county-month of year-day of week fixed effects, the magnitudes on my coefficients of interest all decrease-- indicating that the interaction between

PSL coverage and post-stay-at-home order is correlated with county factors affecting stay-at-home behavior-- though the coefficient values are still positive and significantly different from zero. The coefficients decrease further while remaining positive and significant after including a county linear time trend.

My preferred specification is column 4 because it includes all the relevant fixed effects and a county linear time trend. For this specification, the coefficient on $\text{PostEvent}_{\text{gst}}$ -- the expected additional number of minutes spent at home each day for a person who is not covered by a paid sick leave mandate following the first stay-at-home order in the state relative to the period before the first stay-at-home order-- is 17.67. This means that people who were not covered by paid sick leave mandates are expected to have spent about 17.67 more minutes at home following the first stay-at-home order in their state relative to the time they spent at home before the first stay-at-home order. This estimate is statistically significant at the 5% level. The interaction between $\text{HasPSL}_{\text{gst}}$ and $\text{PostEvent}_{\text{gst}}$ communicates the additional effect of being covered by a paid sick leave mandate relative to not being covered on distancing during the period following the first stay-at-home order in a state. The value of this coefficient is 21.54, which means that people who were covered by a paid sick leave mandate are expected to have spent 21.54 more minutes at home each day relative to those who were not covered by a paid sick leave mandate in the period following the first stay-at-home order in their state. This estimate is significantly different from zero at a 10% level. These results are robust to county linear time trends and clustered standard errors.

In Table 7, I also try out the same specification on a larger time window, from January 1st, 2019 to September 26th, 2020. Because some states gained PSL in January 2020, the coefficient on $\text{HasPSL}_{\text{gst}}$ is not omitted after controlling for census block group fixed effects. The

results show that across the entire time period, people in places with PSL stayed at home, on average, 27 minutes longer than people in places without PSL. People without state-level PSL spent about 24 more minutes at home following the first stay-at-home-order in their state relative to the pre-order period. People in PSL states stayed at home for about 43 more minutes relative to people in non-PSL states in the aftermath of the first stay-at-home order in their states. In sum, the larger time window yields slightly larger estimates for the effect of state stay-at-home orders and the difference between PSL states and non-PSL states following state stay-at-home orders. The higher magnitude may occur because I am averaging over a longer time period and there was a stark jump in distancing from 2019 to 2020. Despite these differences, Table 7's estimates for the interaction between $\text{HasPSL}_{\text{gst}}$ and $\text{PostEvent}_{\text{gst}}$ are still contained within the 95% confidence interval as the estimates in Table 6's results.

As an additional robustness check, in Table 8, I include lagged cases, lagged deaths, plus indicators for a number of other policy events such as non-essential business closures and school closures in states which were occurring around the same time as stay-at-home orders. The results suggest that state school closures increased median home dwell time by about 8 minutes, state restaurant closures increased median home dwell time by about 13 minutes, and state-level state-of-emergency declarations reduced median home dwell time by about 8 minutes. Meanwhile, the coefficient on the interaction between $\text{HasPSL}_{\text{gst}}$ and $\text{PostEvent}_{\text{gst}}$ does not significantly change in this set of results relative to the column 4 interaction estimate in Table 6.

As discussed, the first stay-at-home order in a state is a policy event which occurred at different times depending on the state that someone lived in. Past literature finds that the timing of stay-at-home orders influenced their effects, as earlier orders reaped their jurisdictions greater benefits in terms of increased stay-at-home behavior and reduced case counts and deaths.⁷⁰ It is

⁷⁰ Dave et al., "When do shelter-in-place orders fight COVID-19 best," 29-52

possible that earlier stay-at-home orders in PSL places, possibly due to earlier cases, resulted in these places exhibiting more social distancing, and therefore allowed PSL places to “get ahead of the curve” and stay home before infection rates in their locales increased to a significant degree. Due to the potential identification issues which arise with the state stay-at-home orders, I searched for alternate events where PSL states and non-PSL states were on the similar distancing trends leading into them. The National Emergency Declaration was a federally-spanning event which was relatively exogenous. I examine this event in the following section.

National Emergency Declaration

The National Emergency Declaration, a federal policy event which impacted all individuals in my sample on the same day, occurred on March 13th, 2020. This event reflected a more concerned tone from the White House regarding the COVID-19 pandemic in the United States. Figure 1 suggests that there was a significant behavior change among PSL states and non-PSL states following this event. Table 9 investigates if people in places with state paid sick leave distanced significantly differently relative to people in places without state paid sick leave following the Declaration. I restrict my study period from January 2020 through the end of March 2020 because I hope to focus on the period just before and just after the National Emergency Declaration. For every detail except for the study period, I use the same specifications as the models associated with the first stay-at-home order in the state. I note that after including the full suite of fixed effects and county linear time trends, the coefficients on $\text{HasPSL}_{\text{gst}}$ and $\text{PostEvent}_{\text{gst}}$ are wiped out due to census block group and day fixed effects, respectively. After adding in the fixed effects, the magnitude of the coefficient for $\text{PostEvent}_{\text{gst}} * \text{HasPSL}_{\text{gst}}$ decreases while remaining significantly different from zero.

Like in the previous set of results, my preferred specification is the one in column 4

which includes census block group fixed effects, date fixed effects, county-month of year-day of week fixed effects, and a county linear time trend. The coefficient of interest on the interaction between $\text{HasPSL}_{\text{gst}}$ and $\text{PostEvent}_{\text{gst}}$ is 38.46, which is not statistically different from the coefficient in the previous set of results. This coefficient's value means that people who were covered by a paid sick leave mandate are expected to have spent 38.46 more minutes each day relative to those who were not covered by a paid sick leave mandate in the period following the first stay-at-home order in the state. At a 1% significance level, this estimate is more statistically significant than the previous results' estimates.

I set a larger time window in Table 10. For this set of results, the study period is from January 1st, 2019 to September 26th, 2020. The coefficient on the interaction between $\text{HasPSL}_{\text{gst}}$ and $\text{PostEvent}_{\text{gst}}$ increases to 54.37 and is significant at a 1% level. Even though the coefficient increases, it is not statistically different from the coefficient on the interaction in Table 9.

In Table 11, I include lagged cases, lagged deaths, and indicators for other potentially confounding policy events. The results suggest that school closures increased median home dwell time by about 9 minutes, while restaurant closures increased median home dwell time by about 11 minutes. The coefficient on the interaction between $\text{HasPSL}_{\text{gst}}$ and $\text{PostEvent}_{\text{gst}}$ drops to 31.37 and remains statistically significant at a 1% level.

Overall, the value for the interaction between $\text{HasPSL}_{\text{gst}}$ and $\text{PostEvent}_{\text{gst}}$ produced using the National Emergency Declaration as the event is within the same confidence interval as the interaction value produced using state stay-at-home orders at the events. Based on the difference-in-differences results alone, I still prefer the National Emergency Declaration event because it produces more statistically significant estimates. I can be more confident that results associated with this event are not simply due to chance. Section IV.A.4 will provide more

justification for why I might prefer the National Emergency Declaration event.

4. Event Study

If the baseline specification were an ideal difference-in-differences set up, some exogenous event (such as a state stay-at-home order) would affect one group of states and not others, and we would believe that the group of states that were affected would be an effective comparison group with the group of unaffected states. A perennial concern in the difference-in-differences literature is that a seemingly exogenous event, such as the adoption of a stay-at-home order, is a result of on-the-ground events in the state that are also driving the outcome of interest. The literature around “policy endogeneity” is extensive.⁷¹ Here, it seems likely that local stay-at-home orders may be driven by facts about the pandemic at the time of the order’s effective date, such as rising cases in a state. In order to understand whether states that were early and later adopters of stay-at-home orders were on different paths regarding stay-at-home behaviors even before the orders went into effect, I use an event study. Importantly, for the question here, I also examine whether the trends leading into a stay-at-home order, or the National Emergency Declaration are different for PSL and non-PSL states.⁷² For “first stay-at-home order in state” and “National Emergency Declaration,” the empirical model looks like the following:

$$\text{MedianHomeDwellTime}_{\text{gst}} = \beta_0 + \sum_{t=-14}^{14} \beta_t \text{DaysFromEvent}_{\text{gst}} * \text{HasPSL}_{\text{gst}} + \alpha_g + \gamma_s + \epsilon_{\text{gst}}$$

⁷¹ Besley, Timothy and Ann Case, "Unnatural experiments?"

⁷² While not all shown here, I generated event studies around a number of potentially behavior-influencing events, including the first case in county, first death in country, and CARES enactment date. As described later, only the event study around the National Emergency Declaration showed insignificant pre-trends and sustained, significant coefficients during the post-period.

For each event, my empirical model includes dummy variables for interactions between a state having paid sick leave and a certain number of days before or after an event, from 14 days before an event up until 14 days after an event. The omitted time period is the interaction which is 1 day before the event. I control for CBG, county-month of year-day of week fixed effects, as well as county linear time trends. If there are no differential pre-trends, I expect the coefficients in the “pre-period” to not be statistically different from zero. If being in a state with a paid sick leave mandate has a significant, positive effect on distancing, the coefficients in the “post-period” should be significant and positive.

5. Event Study Results

First Stay-At-Home Order in State

Figure 6 shows event study estimates for the distancing behavior for states without PSL, where the overall study period is restricted from January 1st, 2020-April 30th, 2020. Dots represent the coefficient estimates for the effect of being in a state without PSL on distancing on a day, relative to the effect of being in a state without PSL on distancing on the day before a state’s first stay-at-home order. If a coefficient is not significantly different from zero, its 95% confidence interval--represented by the vertical lines through the dots-- will cross the X axis. As shown, two weeks before their states’ orders, people in states without PSL typically stayed at home less relative to the day before the order. However, they were already beginning to increase their at-home behavior. The week before their states’ orders, the distancing behavior of people in states without PSL was not statistically different from the distancing behavior on the day before their orders. Immediately after stay-at-home orders were effective in their states, people without PSL all stayed at home significantly more.

Figure 7 shows the effect of paid sick leave mandate coverage on social distancing, where dots represent the coefficient estimates for the additional effect of having paid sick leave on distancing on a day, relative to the additional effect of paid sick leave coverage on distancing on the day before a state's first stay-at-home order. Additionally, the overall study period is restricted from January 1st, 2020-April 30th, 2020. The figure shows that throughout the two weeks before and the two weeks following the first stay-at-home order in a state, the difference between PSL states' and non PSL states' at-home behavior was not statistically different from zero. Four days after the first stay-at-home order in a state, the difference in distancing between people in places with paid sick leave relative to those without paid sick leave was positive and more significant, but still not statistically different from zero. Computing constraints meant that I could only run this event study on a 20% sample. It is likely that a larger sample (such as the 50% sample that I ran the baseline models on) would increase the precision of these estimates and lead to more significant results in the post-order-period which are consistent with values reported in Table 6.

These event study results show that, when controlling for factors such as inherent characteristics of places, days of week, and places being on "different paths" prior to an event, there were not significant pre-trends in distancing differences between people in states with paid sick leave relative to others in states without paid sick leave leading up to the first stay-at-home order in a state. Since the difference in distancing between people in states with paid sick leave relative to those without paid sick leave generally was not significant or was weakly significant for the two weeks following the first stay-at-home order in states, the results also suggest that the first stay-at-home order in a state is perhaps not the precise event at which distancing behavior between PLS states and non-PSL states significantly diverged.

National Emergency Declaration

The dots on Figure 8 represent the coefficient estimates for the effect of being a non-PSL state on distancing during the 14 days before and the 14 days after the National Emergency Declaration on March 13th, 2020, relative to the day before the National Emergency Declaration. At first, people in non-PSL states were relatively consistent in their at-home behavior. However, as March 13th approached, people in non-PSL states steadily increased the time that they spent at home each day.

Figure 9 shows the coefficient estimates for the additional effect of having paid sick leave on distancing on a day, relative to the additional effect of paid sick leave coverage on distancing on the day before the National Emergency Declaration. Prior to the National Emergency Declaration, paid sick leave states and non-paid sick leave states exhibited statistically similar levels of distancing--the coefficient on the interaction between day dummies and the “Has PSL” indicator was generally not statistically different from zero (there are a few exceptions where the difference was negative and statistically significant from zero). Immediately following the National Emergency Declaration, this coefficient became positive and significant, save for a few days when it dipped back down in magnitude and lost some significance. As seen in Figure 1, these days coincide with the nationwide drop in at-home behavior following March 19th, the first state stay-at-home order. I posit that this country-wide drop in distancing was due to a “stock-up” rush in anticipation of upcoming days hunkered down at home, which occurred separately from the effect of the National Emergency Declaration.

The overall trend of the event study is consistent with what I would expect to see if the parallel trends assumption holds. Since states with and without PSL seem to be on similar trends leading up to March 13th, the Declaration event does not appear to be subject to endogeneity

bias. Thus, I have greater confidence that the National Emergency Declaration spurred differential distancing between people in places with paid sick leave and people in places without paid sick leave, and that paid sick leave could be accounting for some of this result. It is still possible that fundamental differences between PSL and non-PSL places are driving observed results. But these unaccounted-for factors cannot be characteristics of places that I already control for using CBG fixed effects. Given the validity of my assumptions using the National Emergency Declaration, this is my preferred event. I proceed using this event for subsequent analyses.

B. Regression Discontinuity Design

1. Method

While the baseline results indicate that in the period following the National Emergency Declaration, people in places covered by paid sick leave generally distanced more than people in places without paid sick leave, the results represent differences in average distancing over the entire “pre-event period” relative to average distancing over the entire “post-event period” for PSL and non-PSL groups. They do not show the immediate effect of the National Emergency Declaration on distancing decisions for individuals covered by paid sick leave. A Regression Discontinuity Design (RDD) offers a more granular approach to investigating the link between paid sick leave access and distancing. By focusing on a very short period of time, the RDD helps to simulate the on-the-margin thinking which people underwent when they decided whether or not to attend work or stay home after the National Emergency Declaration. The high-frequency distancing changes which the RDD focuses on are also relatively immune to other factors which influence mobility, making a RDD a useful strategy for testing the conclusions which the baseline results present. Using a RDD, I can also see how significant distancing in places with

paid sick leave translates into lower COVID-19 cases and deaths in those places. Or put differently, I can investigate whether paid sick leave access impacted the course of the disease. Assuming outcomes are changing smoothly for all places immediately before and immediately after the Declaration, the RDD yields the causal effect of the Declaration on behavior, cases, and deaths. And assuming outcomes are changing smoothly and similarly in PSL states, we can see if the causal effect of the Declaration is different in PSL states.

As is common practice with RDDs, I first search for graphical evidence of a jump in PSL states' distancing, county case counts, and county deaths, relative to non PSL states' outcomes following the National Emergency Declaration. Figure 10 is a bin scatter plot which suggests that these discontinuities in distancing exist between places with and without PSL. The red dots are average mobility among people in PSL states, compressed into 10 equally-sized bins on either side of the threshold. The blue dots are average mobility among people in non-PSL states, also compressed into 10 equally-sized bins on either side of the threshold. Prior to generating this figure, I absorbed census block group fixed effects. Figures 11 and 12 which show 5-day lead new cases and 14-day lead new deaths, respectively, follow the same specifications as Figure 10.⁷³

Figure 10 suggests that distancing for people in places with PSL and people in places without PSL increased immediately following March 13th, 2020. PSL states' increase in distancing was larger than the increase observed in non-PSL states. Figure 11 shows that new

⁷³A 5-day lead in cases follows general public health knowledge about the median incubation period of the virus. E.g. if people were exposed on day x , if they were symptomatic, they typically experienced symptoms on day $x+5$. While not all people who became sick with the virus got tested right away (or at all) and were reported in public health data on day $x+5$, this error should not bias my results. This is because I am assuming that testing did not operate differently in PSL states vs non-PSL states. And if this assumption is false, my CBG fixed effects and county linear time trends should pick up differences in testing regimes. Still, since not all cases were associated with a 5-day incubation period, I take caution in my interpretation of cases as an outcome. Similarly, a 14-day death lead is a somewhat arbitrary way of picking up deaths associated with distancing on day X . I also take caution in interpreting the fatality results since the precise days from COVID-19 exposure to COVID-19 death vary immensely across cases.

cases in PSL and non-PSL places rose 5 days following March 13th, but to a lesser extent for PSL places. In Figure 12, we see a greater jump in new deaths for people in places without PSL relative to people in places with PSL 14 days after March 13th. I formalize these graphical results with the following model which uses median home dwell time, 5-day lead new cases in a county, and 14-day lead new deaths in a county as outcome variables:

$$\begin{aligned} \text{Outcome}_{\text{gst}} = & \beta_0 + \beta_1 \text{PostEvent}_{\text{gst}} + \beta_2 \text{DaysSinceEvent}_t + \beta_3 \text{HasPSL}_{\text{gst}} + \\ & \beta_4 \text{PostEvent}_{\text{gst}} * \text{DaysSinceEvent}_t + \beta_5 \text{HasPSL}_{\text{gst}} * \text{DaysSinceEvent}_t + \beta_6 \text{PostEvent}_{\text{gst}} * \text{HasPSL}_{\text{gst}} + \\ & \beta_7 \text{PostEvent}_{\text{gst}} * \text{HasPSL}_{\text{gst}} * \text{DaysSinceEvent}_t + \alpha_g + \gamma_s + \epsilon_{\text{gst}} \end{aligned}$$

DaysSinceEvent_t is the running variable which represents the days since March 13th, 2020. $\text{HasPSL}_{\text{gst}}$ is an indicator for whether or not a CBG on day t is covered by a paid sick leave mandate. $\text{PostEvent}_{\text{gst}}$ is an indicator for whether day t occurs after the March 13th National Emergency Declaration date. Within the RDD framework, this event is considered to be the treatment. The date that $\text{PostEvent}_{\text{gst}}$ corresponds to is the threshold. Since I am controlling for time and $\text{PostEvent}_{\text{gst}}$ turns from 0 to 1 on March 13th, 2020, the β_1 coefficient on $\text{PostEvent}_{\text{gst}}$ represents the “jump” or discontinuity in the outcome which occurs among people not covered by PSL mandates on that day. $\text{PostEvent}_{\text{gst}} * \text{HasPSL}_{\text{gst}}$ accounts for the outcome in PSL places immediately after the National Emergency Declaration on top of the outcome exhibited by people without PSL. The interaction between $\text{PostEvent}_{\text{gst}}$ and DaysSinceEvent_t is included due to potential slope differences on either side of the threshold. I control for CBG and county-month of year-day of week fixed effects, limit my study window to 30 days before and 30 days after the National Emergency Declaration, and cluster standard errors by state.

2. *Potential Pitfalls*

The RDD follows the examples of other works which adapt the traditional Regression

Discontinuity framework to consider time as a running variable and the treatment date as the threshold.⁷⁴ By using the RDD approach and a narrow time window, I am able to hedge off endogeneity concerns because I expect there to be few other events and factors beyond paid sick leave access which explain sharp differences in distancing, cases, and deaths among states occurring at the time of the National Emergency Declaration. I assume that within the short period of time that I am focusing on, there are no other confounding factors which significantly drive observed results.

3. Results

Table 12 reports the results of the RDD which uses distancing as the outcome. The results indicate that people without PSL responded to events on March 13th, 2020 by spending 72.78 more minutes at home relative to the time just before the date. People with PSL spent about 30 more minutes at home relative to people without PSL following March 13th, 2020. Both of these results are significant at a 1% level.

PSL access could be the factor driving this observed jump in distancing. As events on March 13th, 2020 made individuals more aware of COVID-19's impending threat, they may have made decisions regarding how much they would leave their homes and expose themselves to illness. If they were feeling ill or had family members who were feeling ill, the optimal public health decision would have been for them to stay home. Because many individuals in paid sick leave states presumably do not have to worry as much about missing a day of pay for missing a day of work, the greater level of post-event distancing from PSL states could indicate that their decisions to stay home were made easier due to state-level paid sick leave access.

⁷⁴ Brodeur et al., "COVID-19, lockdowns and well-being"; Caselli, Francesca, Francesco Grigoli, Damiano Sandri, and Antonio Spilimbergo. *Mobility under the COVID-19 Pandemic*; Hausman, Catherine, and David S. Rapson, "Regression discontinuity in time"

Table 13 shows how distancing following March 13th, 2020 may have translated into new county cases 5 days later. Based on the results of this table, it seems that places covered by PSL mandates experienced fewer cases than places not covered by PSL mandates 5 days after presumed exposure on March 13th, 2020. This follows previous results that show that places covered by PSL mandates distanced more, and suggests that greater levels of social distancing may have translated into decreased virus transmission and fewer cases. However, these results are not statistically different from zero. The results are also not statistically different from zero when I change the outcome to cases which occurred 14 days after presumed exposure.⁷⁵

Table 14 demonstrates how distancing on March 13th, 2020 may have translated into COVID-19 deaths 14 days later. Like the results for the cases, places with PSL mandates experienced fewer deaths 14 days later than places not covered by PSL mandates. While this suggests that increased distancing in PSL places may have translated into fewer deaths, the results are not statistically significant.

The event study and RDD make clear that the National Emergency Declaration was a consequential event which drove differential distancing between PSL and non-PSL places. These specifications also indicate that the parallel trends assumption holds using this event. I will now redo the baseline specification around the National Emergency Declaration and see if there are heterogeneous effects depending on characteristics of the people in the states that I am studying. We might, for example, expect bigger effects of state-mandated PSLs in places where there are more people who are unlikely to have previous paid sick leave coverage.

C. Baseline Heterogeneous Effects

1. Method

Identifying assumptions of my empirical strategy include that paid sick leave alone

⁷⁵ See Appendix Table 2

accounts for the distancing differences between PSL and non-PSL places and that the effects observed through the results occur uniformly for all people. However, these assumptions may not be entirely true. In states with and without PSL mandates, people may already receive paid sick leave through their employer. For example, I expect that college-educated individuals are more likely to work in occupations that receive PSL from employers. For these people, I do not expect state-level PSL mandates to have a significant impact on stay-at-home behavior. Meanwhile, if there is a large sum of people in a state who do not receive PSL through their employers, such as service sector employees, I would expect that PSL mandates have a greater effect on their stay-at-home behavior.

To test these hypotheses, I interact potential confounders with existing interactions between $\text{PostEvent}_{\text{gst}}$ and $\text{HasPSL}_{\text{gst}}$ from the baseline specification which uses the National Emergency Declaration as the event. If my original coefficient values change and/or the coefficients on the new interactions are significant, the identifying assumptions may be violated. I examine three heterogeneous effects- “share non-Hispanic Black”, “share Hispanic”, and “share college-educated.” The model for “share non-Hispanic Black” looks like the following:

$$\begin{aligned} \text{MedianHomeDwellTime}_{\text{gst}} = & \beta_0 + \beta_1 \text{ShareBlack}_{\text{gst}} + \beta_2 \text{PostEvent}_{\text{gst}} + \beta_3 \text{HasPSL}_{\text{gst}} + \\ & \beta_4 \text{PostEvent}_{\text{gst}} * \text{ShareBlack}_{\text{gst}} + \beta_5 \text{HasPSL}_{\text{gst}} * \text{ShareBlack}_{\text{gst}} + \beta_6 \text{PostEvent}_{\text{gst}} * \text{HasPSL}_{\text{gst}} + \\ & \beta_7 \text{PostEvent}_{\text{gst}} * \text{HasPSL}_{\text{gst}} * \text{ShareBlack}_{\text{gst}} + \alpha_g + \gamma_s + \omega_t + \omega_t * \text{ShareBlack}_{\text{gst}} + \epsilon_{\text{gst}} \end{aligned}$$

which has each of the same variables and interactions as the baseline specification, a main effect for “share non-Hispanic Black” and interactions between “share non-Hispanic Black” and parameters from the baseline specification. My coefficient of interest is β_7 , which shows the effect of a 1 percentage point increase in Non-Hispanic Black people in a census block group on distancing, in addition to the effect of having PSL and the effect of being in the “post period” of

the National Emergency Declaration.

2. Results

Table 15 reports heterogeneous effects associated with the effect of paid sick leave on distancing following the National Emergency Declaration. It appears that, following the National Emergency Declaration, a one percentage point increase in the share of college-educated individuals within a CBG that is in a PSL state was associated with people within that CBG spending, on average, 36 additional minutes at home. Meanwhile, a one percentage point increase in the share of non-Hispanic Black people had no statistically significant additional effect on distancing in a place covered by PSL following the Declaration. A one percentage point increase in the share of Hispanic people within a CBG that was covered by paid sick leave was associated with people within that CBG spending, on average, 49 fewer minutes at home following the Declaration. While the difference in distancing between PSL states and non-PSL states remains positive and significant after the inclusion of these interactions, the results show that the presence of high numbers of college-educated individuals within a CBG covered by PSL increased the estimated level of distancing in that jurisdiction, while high numbers of Hispanics within a CBG covered by PSL essentially washed out this difference.

Consistent with past works examining heterogeneous stay-at-home order and social distancing compliance, these results suggest that educational level and ethnicity mattered in influencing who could stay home during the pandemic, even when paid sick leave mandates were on the table. There are several possible explanations for the observed results.

College-educated individuals are more likely to be in a higher-income bracket and work in jobs where an employer offers them paid sick leave. Their jobs may be easily done from home. Thus, as a group, college-educated individuals likely will have an opportunity to stay at home more

relative to others, whether or not their state has a sick leave mandate. If high numbers of college-educated individuals live in states which pass PSL, people in these states are more likely to respond to a pandemic by staying home. Meanwhile, Hispanic people disproportionately make up front-line service jobs. As previously noted, they are also less likely to have access to sick leave through their employers. It appears contradictory that this group did not respond to sick leave coverage within PSL states by distancing more during the pandemic. This result could imply that there exist sustained coverage and effectiveness gaps for paid sick leave mandates which were passed in states. See Section III.E for more information on limitations of PSL mandates.

D. FFCRA Modification

1. Method

A drawback of my earlier analysis is that there exists no variation over time in who is covered by state-mandated paid sick leave law. Thus, while I find that people in places that had state-mandated paid sick leave laws responded differently to pandemic related events, it is possible that these are just the kind of people who respond differently to information. To better understand whether extending paid sick leave to those who did not previously have it affects stay-at-home behavior, I shift my focus to a different event: the effective date of the FFCRA. The FFCRA, which went into effect on April 1st, 2020, was a federal law that extended two weeks of paid sick leave to all workers, whether or not they were covered by a state PSL mandate. Unlike state-mandated PSL laws which often set waiting periods and accrual rates, the FFCRA's sick time was available immediately on April 1st, 2020. In an unreported analysis, Andersen et al. (2020) implemented a “reverse” difference-in-differences approach where the people in states that had state, county, and city PSL laws on-the-books formed the control group

and the people who had paid sick leave newly extended to them through the FFCRA formed the treatment group. The authors were looking for whether or not the FFCRA led to differential distancing among people with and without previous state-level PSL access. They state that their findings for this specification were inconclusive. They believe that one of the culprits for the inconclusive results was the variation across exact terms for the FFCRA, state, county, and city PSL laws.

I replicate the study authors' unreported analysis below, though I only count CBGs as being covered by a PSL mandate if the state where they are located in had a PSL law in effect, rather than accounting for city, county, and state laws. I assume that the research team aggregated CBGs up to counties, as they did for the rest of their study. Unlike their aggregation approach, I leave jurisdictions at the CBG level and am therefore able to control for static, geographic variation on a more granular level. In this specification, I force “Has PSL” to be the omitted category. My model takes the following form:

$$\text{MedianHomeDwellTime}_{\text{gst}} = \beta_0 + \beta_1 \text{PostFFCRA}_{\text{gst}} + \beta_2 \text{NoPSL}_{\text{gst}} + \beta_3 \text{PostFFCRA}_{\text{gst}} * \text{NoPSL}_{\text{gst}} + \alpha_g + \gamma_s + \omega_t + \varepsilon_{\text{gst}}$$

where $\text{PostFFCRA}_{\text{gst}}$ indicates 1 if the FFCRA is in effect and 0 otherwise. $\text{NoPSL}_{\text{gst}}$ is another indicator variable which is 1 if a census block group on day t is not covered by state-mandated PSL and 0 otherwise. The interaction between $\text{PostFFCRA}_{\text{gst}}$ and $\text{NoPSL}_{\text{gst}}$ indicates 1 for a day which follows April 1st in a CBG that is not covered by a state-level paid sick leave mandate. This interaction is otherwise set to 0.

The coefficient β_3 on the interaction between $\text{PostFFCRA}_{\text{gst}}$ and $\text{NoPSL}_{\text{gst}}$ estimates the expected effect on of being in a state not covered by a paid sick leave mandate in addition to the effect of being in the “post period” of the FFCRA effective date, on minutes spent at home each

day. A positive coefficient would mean that I expect people in places not covered by paid sick leave to, on average, stay at home more relative to people in places covered by paid sick leave after the FFCRA effective date. A negative coefficient would mean that I expect people in places not covered by paid sick leave to, on average, stay at home less relative to people in places covered by paid sick leave after the FFCRA effective date.

I construct the model in this way because the FFCRA was a federal paid sick leave policy which covered individuals across all 50 states. People who already accessed paid sick leave through state mandates could also receive sick leave through the FFCRA. If paid sick leave laws were effective ways to keep people at home, controlling for a variety of other distancing-influencing factors, I expect to see people who newly gained access to paid sick leave following the effective date of the FFCRA to distance more relative to people who already had PSL access prior to the FFCRA.

The intuition of this is that I do not expect the FFCRA to be felt as a major policy impact on those who were already covered by paid sick leave mandates unless those with paid sick leave mandates had already used up their allotted paid sick leave for the year or the mandates in their states were not sufficient in allowing them to stay home during the pandemic. If the FFCRA did not significantly impact those covered by on-the-books PSLs, the difference in this group's distancing between the pre and post period of the FFCRA effective date should be nominal. Meanwhile, following the FFCRA effective date, people in places without previous access to PSL would be experiencing a new benefit. The effect of the policy treatment should be greater for them relative to the effect of the policy treatment on those already covered by paid sick leave. Seeing a positive difference in distancing between these two kinds of places also would reinforce the importance of paid sick leave for promoting distancing during a pandemic. A positive

difference would suggest that it takes receiving paid sick leave for formerly non-PSL places to “catch up” to the level of distancing exhibited by people who already had paid sick leave.

2. Potential Pitfalls

Challenges associated with the FFCRA modification are similar to the ones which the two baseline specifications pose--namely parallel trends violations and endogeneity concerns. To combat these challenges, I include county linear time trends and CBG, county-month of year-day of week, and day fixed effects. I also try adding lagged cases and indicators for other policy events to see if my coefficients change. See Section IV.D.5 which investigates whether differential pre-trends are driving the results.

3. Results

In Table 16, I show the results for the FFCRA modification. These results demonstrate the effect of the federal paid sick leave mandate on distancing among people who did not have prior access to paid sick leave through their states. I restrict my study period from March 2020 through the end of April 2020 in order to focus on a window of time before and after the policy effective date. As shown in Tables 17 and 18, the results are not robust to alternate time windows. Tables 17 and 18 make the precise effect of the FFCRA less certain. They could suggest that people distanced differently in the short-run of the FFCRA effective date relative to the long-run. Though if that is the case, it is unclear if the discrepancy was driven by people in PSL states distancing more in the long-run, people in non-PSL states distancing less in the long-run, or a combination of the two. Just like the baseline approach, column 1 is the specification without any fixed effects and columns 2-4 include census block group fixed effects, date fixed effects, and county-month of year-day of week fixed effects. Adding in CBG fixed effects wipes out the “no paid sick leave” coefficients because these fixed effects control for

static differences across places, such as whether or not a person has access to a state-level paid sick leave mandate. Adding in date fixed effects wipes out the “post FFCRA effective date” coefficients because the FFCRA became effective federally on the same day.

The coefficient on the interaction between $\text{PostFFCRA}_{\text{gst}}$ and $\text{NoPSL}_{\text{gst}}$ is not as well-behaved as the coefficient on the interaction from the two baseline specifications-- the coefficient increases after I add county-month of year-day of week fixed effects and is no longer statistically significant from zero. After I include the county linear time trend, the coefficient increases further and is statistically significant at a 1% level.

In column four, my preferred specification, the coefficient on the interaction between $\text{PostFFCRA}_{\text{gst}}$ and $\text{NoPSL}_{\text{gst}}$ communicates the effect of obtaining paid sick leave through the FFCRA on distancing for people who did not have prior access to state mandates relative to those who were already covered by state-level PSL mandates. The value of this coefficient is 23.40, which means that people who were not covered by a paid sick leave mandate are expected to have spent about 23 more minutes at home each day relative to those who were already covered by a paid sick leave mandate in the period following the FFCRA effective date. This estimate is significant at a 1% level. These results are robust to county linear time trends and clustered standard errors. Table 19 shows that after including lagged cases, lagged deaths, and indicators for other policy events, this coefficient does not significantly change.

These results suggest that gaining access to PSL may have “closed the gap” in distancing behavior between people who already had PSL access relative to those who did not, at least in the short-run. In fact, the gap more than just closed-- after including county linear time trends, it appears that those without prior access to paid sick leave through state-level mandates actually distanced more than those with paid sick leave mandates after gaining paid sick leave benefits

through the FFCRA.

As I did for the baseline specifications, I implemented event studies in order to investigate if the parallel trends assumption holds for the FFCRA modification.

4. Event Study

For “FFCRA effective date”, the empirical model for the event study looks like the following:

$$\text{MedianHomeDwellTime}_{\text{gst}} = \beta_0 + \sum_{t=-14}^{14} \beta_t \text{DaysFromEvent}_{\text{gst}} * \text{NoPSL}_{\text{gst}} + \alpha_g + \gamma_s + \varepsilon_{\text{gst}}$$

The primary difference between this event study and the event study for the other two baseline events is that I interact each day dummy with $\text{NoPSL}_{\text{gst}}$. If the FFCRA had a significant, positive effect on distancing that was separate from the effect of the state-level PSL mandates, the coefficients in the “pre-period” for this event study should not be statistically different from zero and the coefficients in the “post-period” should be significant and positive.

5. Event Study Results

Figures 13 and 14 show the relevant results for the FFCRA event study. Figure 13 shows that distancing behavior was generally on the rise for places without PSL prior to the FFCRA effective date. After April 1st, the distancing behavior among people covered by state-level PSL mandates stayed relatively constant. People in states covered by PSL mandates generally stayed at home more in this post-period relative to the time just before just before the FFCRA went into effect.

Figure 14 shows that throughout the 14 days before and 14 days following the FFCRA effective date, the coefficient on the interaction between $\text{NoPSL}_{\text{gst}}$ and $\text{DaysFromEvent}_{\text{gst}}$ fluctuated in terms of sign, magnitude, and significance. 14 days before the FFCRA effective date, the difference between PSL and non-PSL states’ stay-at-home behavior was already greater

relative to the difference in at-home behavior on the day before the FFCRA effective date. Over time, the coefficient approached the value of the coefficient on the day before the FFCRA effective date and was not statistically different from zero. Following the effective date, the coefficient of the interaction between $\text{NoPSL}_{\text{gst}}$ and $\text{DaysFromEvent}_{\text{gst}}$ was generally more positive and significant relative to the days before the effective date. This figure shows that, after controlling for a suite of factors and accounting for places being on “different paths” prior to the FFCRA effective date, there were some early, significant pre-trends in distancing differences between people in places with paid sick leave relative to others in places without paid sick leave leading up to the FFCRA effective date. This early period coincided with other behavior-influencing policy events, such as the National Emergency Declaration and state-level stay-at-home orders. After the FFCRA effective date, the differences in distancing between places without PSL and places with PSL were generally positive and statistically different from zero.

This event study analysis adds some endogeneity concerns to my results in Table 16. Because there were some significant pre-trends and post trends which were not all statistically different from zero, it is more difficult to believe that there was a causal link between the FFCRA and non-PSL states exhibiting more distancing. Still, I proceed with some tests for heterogeneous effects to see what other factors may be contributing to the estimated distancing behavior.

E. FFCRA Modification Heterogeneous Effects

1. Method

I interact potential confounders with existing interactions between $\text{PostEvent}_{\text{gst}}$ and $\text{HasPSL}_{\text{gst}}$, where $\text{PostEvent}_{\text{gst}}$ corresponds with “FFCRA effective date.” This process clues me

into whether or not the observed distancing outcomes for PSL states and non-PSL states occur uniformly for all people within those states. As I did for the baseline models, I tried out three heterogeneous effects- “share non-Hispanic Black”, “share Hispanic”, and “share college-educated.” The model for “share non-Hispanic Black” looks like the following:

$$\begin{aligned} \text{MedianHomeDwellTime}_{\text{gst}} = & \beta_0 + \beta_1 \text{ShareBlack}_{\text{gst}} + \beta_2 \text{PostEvent}_{\text{gst}} + \beta_3 \text{NoPSL}_{\text{gst}} + \\ & \beta_4 \text{PostEvent}_{\text{gst}} * \text{ShareBlack}_{\text{gst}} + \beta_5 \text{NoPSL}_{\text{gst}} * \text{ShareBlack}_{\text{gst}} + \beta_6 \text{PostEvent}_{\text{gst}} * \text{NoPSL}_{\text{gst}} + \\ & \beta_7 \text{PostEvent}_{\text{gst}} * \text{NoPSL}_{\text{gst}} * \text{ShareBlack}_{\text{gst}} + \alpha_g + \gamma_s + \omega_t + \omega_t * \text{ShareBlack}_{\text{gst}} + \varepsilon_{\text{gst}} \end{aligned}$$

which has each of the same variables and interactions as the FFCRA modification, a main effect for “share non-Hispanic Black” and interactions between “share non-Hispanic Black” and parameters from the FFCRA modification. My coefficient of interest is β_7 , which shows the effect of a 1 percentage point increase in non-Hispanic Black people in a census block group on distancing, in addition to the effect of having PSL and the effect of being in the “post period” of the FFCRA effective date.

2. Results

Table 20 reports heterogeneous effects associated with the effect of paid sick leave on distancing following the FFCRA effective date. The results show that, following the FFCRA effective date, a one percentage point increase in Hispanic people within a CBG that was not covered by paid sick leave was associated with people within that CBG spending, on average, 42 more minutes at home. Despite this significant interaction, the difference in distancing between PSL states and non-PSL states remained positive and significant. We may interpret this result as an indication that the FFCRA allowed certain groups, such as Hispanic people in places without state paid sick leave mandates, to access paid sick leave and distance more relative to the time

before the FFCRA effective date. However, prior endogeneity concerns still temper my trust in FFCRA as an empirically valid, exogenous policy event within this reverse differences-in-differences framework.

V. Conclusion

This paper offers a new perspective to COVID-19 literature by focusing on the role of already-on-the-books, state-level paid sick leave policies on enabling people to stay at home and avoid illness, particularly during the early months of the virus' spread. In March, as cases and deaths rose in all states and the federal government's announcements made the Coronavirus pandemic's threat more salient, people all over the country began to stay at home more. My results show that this increased stay-at-home behavior was more pronounced for people living in states covered by PSL. In particular, I estimate that individuals covered by state-level sick leave mandates stayed at home about 30 minutes more than people not covered by sick leave mandates following the National Emergency Declaration on March 13th, 2020. This jump in distancing did not lead to significantly fewer cases and deaths in states covered by paid sick leave. The education levels and ethnicities of people in PSL states were associated with further differences in distancing during the post-Declaration period. I do not find fully conclusive results that newly-gained access to PSL through the FFCRA led to greater distancing for people in non-PSL states. Overall, these results suggest that paid sick leave policies could be important contributors towards population-level social distancing during a pandemic, though their estimated impacts are small in magnitude. Future works should investigate these state laws with greater nuance-- how do "skimpy" mandates influence the level of distancing which people in PSL states exhibit? And are the individuals most eligible for coverage under the sick leave mandates distancing more?

Additionally, though I implement several robustness checks and tests for assumption

violations, prevailing endogeneity concerns still prevent me from directly drawing a direct causal link between PSL mandate access and increased social distancing. Other aspects of states whose citizens vote for elected officials that pass paid sick leave mandates into law could be causing increased distancing in those places. Without a natural experiment setting, it is difficult to achieve an identification strategy which yields a causal estimate.

Despite these limitations, I maintain that the intention of this study is important. By evaluating the effectiveness of paid sick leave mandates in preventing illness spread at the commencement of a global pandemic, a time when more people are contracting illness and facing the decision of whether or not to stay home from work, policymakers can better equip societies with public health tools that *do work* in preventing devastating human health effects. Even if PSL mandates are not antidotes to a public health crisis like COVID-19, researchers and practitioners should continue to search for optimal policies that do ensure that people stay home, tend to their illnesses, take care of loved ones, and limit future spread of infection.

VI. Tables & Figures

Table 1
Median Home Dwell Time in CBG, by State Has PSL

	Has PSL	No PSL	All				
	Mean	Mean	Mean	S.E.	Min	Max	N
Median Home Dwell Time	666.328	635.428	644.006	199.429	0	1498	47439780

Notes: Data source is SafeGraph Social Distancing Metrics (v2) from January 1st, 2019-December 31st, 2019. Unit of observation is a census block group on a day. Statistics generated using a 60% random subsample of census block groups.

Table 2
Median Home Dwell Time in CBG, by Day of Week

Day of Week	Mean	S.E.	Min	Max	N
Sunday	735.675	237.802	0	1498	6758544
Monday	637.59	187.09	0	1439	6758544
Tuesday	629.274	182.1	0	1439	6888516
Wednesday	626.676	180.84	0	1439	6758544
Thursday	619.102	178.999	0	1439	6758544
Friday	589.387	173.725	0	1439	6758544
Saturday	670.623	213.578	0	1439	6758544

Notes: Data source is SafeGraph Social Distancing Metrics (v2) from January 1st, 2019-December 31st, 2019. Unit of observation is a census block group on a day. Statistics generated using a 60% random subsample of census block groups.

Table 3
Median Home Dwell Time in CBG, by Month of Year

Month of Year	Mean	S.E.	Min	Max	N
January	658.433	196.75	0	1439	4029132
February	644.138	190.073	0	1439	3639216
March	606.089	195.24	0	1439	4029132
April	602.391	184.636	0	1438	3899160
May	586.159	193.312	0	1438	4029132
June	602.867	197.989	0	1439	3899160
July	646.503	198.284	0	1439	4029132
August	657.685	196.425	0	1439	4029132
September	658.124	204.044	0	1439	3899160
October	671.626	183.94	0	1439	4029132
November	703.584	193.318	0	1498	3899160
December	690.198	217.511	0	1439	4029132

Notes: Data source is SafeGraph Social Distancing Metrics (v2) from January 1st, 2019-December 31st, 2019. Unit of observation is a census block group on a day. Statistics generated using a 60% random subsample of census block groups.

Table 4
Demographic Group Shares in CBG, by State Has PSL

Demographic Group	No PSL	Has PSL		
Share	(Mean)	(Mean)	Difference	Difference Stat Sig?
Female	0.514	0.509	-0.005	***
Male	0.486	0.491	0.005	***
Non-Hispanic White	0.654	0.566	-0.088	***
Hispanic	0.045	0.085	0.04	***
Black	0.147	0.087	-0.06	***
Indigenous	0.006	0.009	0.003	***
Asian	0.031	0.086	0.055	***
HawPI	0.001	0.003	0.002	***
Mixed-Race	0.021	0.027	0.006	***
Other Race	0.002	0.003	0.001	***
HS Degree or less	0.139	0.132	-0.007	***
College Grad	0.272	0.327	0.055	***
>65 y.o	0.287	0.281	-0.006	***
Private Worker	0.714	0.711	-0.003	***
Self-Employed	0.096	0.105	0.009	***
Non-Profit Worker	0.08	0.08	0	
Government Worker	0.144	0.142	-0.002	***
Speak English	0.988	0.981	-0.007	***
Earns>200K	0.045	0.077	0.032	***

Works from Home	0.041	0.051	0.01	***
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*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source is 2016 American Community Survey 5-Year Estimates. Unit of observation is a census block group. Statistics generated using a 1% random subsample of census block groups.

Table 5
States With On-the-Books PSL, April 2021

State	Law Effective Date
Arizona	Jul 1, 2017
California	Jan 1, 2015
Colorado	Jan 1, 2021
Connecticut	Jan 1, 2012
Maine*	Jan 1, 2021
Massachusetts	Jul 1, 2015
Maryland	Feb 11, 2018
Michigan	Mar 29, 2019
New Jersey	Oct 29, 2018
Nevada*	Jan 1, 2020
New York	Jan 1, 2021
Oregon	Jan 1, 2016
Rhode Island	Jul 1, 2018
Vermont	Jan 1, 2017
Washington	Jan 1, 2018
Washington, D.C.	Nov 13, 2008

*Paid Time Off

Notes: Source for paid sick leave law by states is A Better Balance. Effective dates retrieved from state government websites and paid sick leave laws. For the purposes of my study, I consider paid time off laws to be functionally equivalent to paid sick leave laws.

Table 6
Effect of First Stay-at-Home Order in Each State on Median Home Dwell Time in CBG, by
State Has PSL
Time Window: January 1st, 2020 -April 30th, 2020

VARIABLES	(1)	(2)	(3)	(4)
state has PSL	20.14* (10.91)	-	-	-
post 1st state stay-at-home order	190.2*** (13.33)	35.33*** (11.71)	20.01* (10.87)	17.67** (8.732)
state has PSL X post first state stay-at-home order	73.39*** (16.28)	70.96*** (16.62)	33.03** (13.95)	21.54* (12.64)
Observations	13,105,752	13,105,752	13,105,752	13,105,752
Adjusted R-squared	0.112	0.690	0.707	0.708
Census Block Group f.e.		X	X	X
Date f.e.		X	X	X
County X Month Of Year X Day Of Week f.e.			X	X
County Trend				X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from January 1st, 2020-April 30th, 2020. Data source for stay-at-home orders is Boston University COVID-19 US State Policy Database. Unit of observation is a census block group on a day. Results generated using a 50% random subsample of census block groups. “state has PSL” is omitted beginning in column 2 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states.

Table 7
Effect of First Stay-at-Home Order in Each State on Median Home Dwell Time in CBG, by
State Has PSL
Time Window: January 1st, 2019-September 26th, 2020

VARIABLES	(1)	(2)	(3)	(4)
state has PSL	28.47** (11.70)	-23.99*** (6.490)	-14.20** (5.874)	26.55*** (8.085)
post 1st state stay-at-home order	69.34*** (9.214)	36.76*** (10.38)	31.27*** (10.49)	23.93*** (7.439)
state has PSL X post first state stay-at-home order	48.96*** (17.14)	44.53*** (16.22)	38.70** (14.84)	43.46*** (12.13)
Observations	68,778,120	68,778,120	68,778,120	68,778,120
Adjusted R-squared	0.034	0.555	0.569	0.579
Census Block Group f.e.		X	X	X
Date f.e.		X	X	X
County X Month Of Year X Day Of Week f.e.			X	X
County Trend				X

Robust standard errors in
parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from January 1st, 2019-September 26th, 2020. Data source for stay-at-home orders is Boston University COVID-19 US State Policy Database. Unit of observation is a census block group on a day. Results generated using a 50% random subsample of census block groups.

Table 8
Effect of First Stay-at-Home Order in Each State on Median Home Dwell Time in CBG, by
State Has PSL
Time Window: January 1st, 2020 -April 30th, 2020

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
state has PSL	-	-	-	-	-	-	-	-
post first state stay-at-home order	17.61* (8.909)	15.87* (8.246)	15.75* (8.082)	17.88** (8.725)	16.80* (8.727)	14.39 (9.660)	15.10 (9.219)	11.24 (8.586)
state has PSL X post first state stay-at-home order	21.94* (12.76)	23.12* (11.95)	23.43* (11.88)	22.94* (12.68)	20.39 (13.20)	21.73* (12.72)	23.04* (12.93)	23.78* (12.24)
7-days lagged cumulative cases in state		-0.00021 2*** (6.20e-05)						-0.00010 3 (0.00047 3)
7-days lagged cumulative deaths in state			-0.00285 *** (0.00070 1)					-0.00144 (0.00596)
state declares state of emergency					-10.84** (4.201)			-8.185* (4.283)
state closes schools				9.662** (4.491)				8.438** (3.603)
state closes non-essential biz						4.576 (6.400)		2.936 (6.401)
state closes restaurants							15.00*** (4.480)	12.79*** (4.692)
Observations	15,726,6 12	15,726,6 12	15,726,6 12	15,726,6 12	15,726,6 12	15,726,6 12	15,726,6 12	15,726,6 12
Adjusted R-squared	0.709	0.709	0.709	0.709	0.709	0.709	0.709	0.709
Controls	X	X	X	X	X	X	X	X

Robust standard errors in

parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from January 1st, 2020-April 30th, 2020. Data source for stay-at-home orders, state of emergency in states, school closures, nonessential business closures, and restaurant closures is Boston University COVID-19 US State Policy Database. Data sources for cases and deaths are The COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts. Unit of observation is a census block group on a day. Results generated using a 60% random subsample of census block groups. Controls include census block group fixed effects, date fixed effects, county-month of year-day of week fixed effects, and county linear time trend. “state has PSL” is omitted beginning in column 1 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states.

Table 9
Effect of National Emergency Declaration (March 13th, 2020) on Median Home Dwell
Time in CBG, by State Has PSL
Time Window: January 1st, 2020 -March 31st, 2020

VARIABLES	(1)	(2)	(3)	(4)
state has PSL	24.46** (10.31)	-	-	-
post National Emergency Declaration	77.52*** (9.671)	-	-	-
state has PSL X post National Emergency Declaration	80.13*** (20.72)	80.13*** (20.72)	51.00*** (11.80)	38.46*** (9.727)
Observations	9,856,392	9,856,392	9,856,392	9,856,392
Adjusted R-squared	0.031	0.687	0.701	0.703
Census Block Group f.e.		X	X	X
Date f.e.		X	X	X
County X Month Of Year X Day Of Week f.e.			X	X
County Trend				X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from January 1st, 2020-March 31st, 2020. Unit of observation is a census block group on a day. Results generated using a 50% random subsample of census block groups. “state has PSL” is omitted beginning in column 2 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states. “post National Emergency Declaration” is omitted beginning in column 2 because this was a federal event which is absorbed by day fixed effects.

Table 10
Effect of National Emergency Declaration (March 13th, 2020) on Median Home Dwell
Time in CBG, by State Has PSL
Time Window: January 1st, 2019-September 26th, 2020

VARIABLES	(1)	(2)	(3)	(4)
state has PSL	29.96** (11.68)	-26.40*** (6.727)	-17.36** (6.885)	29.03*** (7.854)
post National Emergency Declaration	68.85*** (8.071)	-	-	-
state has PSL X post National Emergency Declaration	49.05*** (16.85)	50.52*** (16.50)	43.15*** (14.60)	54.37*** (13.10)
Observations	68,778,120	68,778,120	68,778,120	68,778,120
Adjusted R-squared	0.036	0.555	0.569	0.579
Census Block Group f.e.		X	X	X
Date f.e.		X	X	X
County X Month Of Year X Day Of Week f.e.			X	X
County Trend				X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from January 1st, 2019-September 26th, 2020. Unit of observation is a census block group on a day. Results generated using a 50% random subsample of census block groups. “post National Emergency Declaration” is omitted beginning in column 2 because this was a federal event which is absorbed by day fixed effects.

Table 11
Effect of National Emergency Declaration (March 13th, 2020) on Median Home Dwell
Time in CBG, by State Has PSL
Time Window: January 1st, 2020 -March 31st, 2020

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
state has PSL	-	-	-	-	-	-	-	-	-
post National Emergency Declaration	-	-	-	-	-	-	-	-	-
state has PSL X post National Emergency Declaration	38.92** *	38.98** *	38.45** *	39.14** *	37.26** *	34.39** *	35.55** *	33.75** *	31.37** *
	(9.770)	(9.431)	(9.570)	(9.800)	(10.34)	(9.702)	(9.858)	(9.701)	(9.799)
7-day lagged cumulative cases in state		0.00315 *** (0.0004 27)							0.00402 (0.0041 8)
7-day lagged cumulative deaths in state			0.249** * (0.0267)						-0.103 (0.350)
state closes schools				6.270 (4.518)					8.611* (4.937)
state declares state of emergency					-6.215 (4.714)				-1.074 (3.633)
state closes non-essential biz						19.76** (8.931)			12.22 (8.361)
state closes restaurants							13.09** (5.022)		10.71** (4.608)
state institutes stay-at-home order								19.78** (9.758)	8.770 (9.590)
Observations	11,827, 452	11,827, 452	11,827, 452	11,827, 452	11,827, 452	11,827, 452	11,827, 452	11,827, 452	11,827, 452
Adjusted R-squared	0.704	0.704	0.704	0.704	0.704	0.704	0.704	0.704	0.704

Controls	X	X	X	X	X	X	X	X	X
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Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from January 1st, 2020-March 31st, 2020. Data source for stay-at-home orders, state of emergency in states, school closures, nonessential business closures, and restaurant closures is Boston University COVID-19 US State Policy Database. Data sources for cases and deaths are The COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts. Unit of observation is a census block group on a day. Results generated using a 60% random subsample of census block groups. Controls include census block group fixed effects, date fixed effects, county-month of year-day of week fixed effects, and county linear time trend. “state has PSL” is omitted beginning in column 1 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states. “post National Emergency Declaration” is omitted beginning in column 1 because this was a federal event which is absorbed by day fixed effects.

Table 12
Regression Discontinuity Design: Immediate Effect of National Emergency Declaration
(March 13th, 2020) On Median Home Dwell Time in CBG, By State Has PSL
Time Window: February 12th 2020, 2020 -April 13th, 2020

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
state has PSL X post National Emergency Declaration			65.89*** (13.68)	37.44*** (10.29)	37.44*** (10.29)	30.09*** (8.817)
days since National Emergency Declaration (running variable)	4.203*** (0.162)	-0.784*** (0.212)	-0.784*** (0.212)	-1.162*** (0.168)	-1.162*** (0.168)	-2.005*** (0.175)
post National Emergency Declaration (threshold)	93.74*** (6.322)	83.25*** (6.474)	63.83*** (7.274)	72.21*** (5.688)	72.21*** (5.688)	72.78*** (4.178)
state has PSL	71.63*** (22.89)	71.63*** (22.89)	39.22** (17.86)	58.49*** (21.83)	-	-
days since National Emergency Declaration X post National Emergency Declaration		10.49*** (0.254)	10.49*** (0.254)	10.69*** (0.214)	10.69*** (0.214)	11.02*** (0.365)
state has PSL X days since National Emergency Declaration				1.285*** (0.341)	1.285*** (0.341)	0.629** (0.295)
state has PSL X days since National Emergency Declaration X post National Emergency Declaration				-0.693 (0.580)	-0.693 (0.580)	1.175** (0.529)
Observations	7,928,292	7,928,292	7,928,292	7,928,292	7,928,292	7,928,058
Adjusted R-squared	0.146	0.167	0.170	0.170	0.699	0.724
Census Block Group f.e.					X	X
County X Month Of Year X Day Of Week f.e.						X

Robust standard errors in
parentheses

*** $p < 0.01$, ** $p < 0.05$, *
 $p < 0.1$

Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from February 12th, 2020-April 13th, 2020. Results generated using a 60% random subsample of census block groups. “state has PSL” is omitted beginning in column 5 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states.

Table 13
Regression Discontinuity Design: Immediate Effect of National Emergency Declaration
(March 13th, 2020) On New, 5-Day-Lead Cases in County, By State Has PSL
Time Window: February 12th 2020, 2020 -April 13th, 2020

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
state has PSL X post National Emergency Declaration			14.93 (55.03)	-15.37 (35.87)	-15.37 (35.87)	-26.72 (26.54)
days since National Emergency Declaration (running variable)	1.880*** (0.477)	0.194*** (0.0651)	0.194*** (0.0651)	0.156* (0.0868)	0.156* (0.0868)	0.110** (0.0486)
post National Emergency Declaration (threshold)	43.95* (25.62)	40.41 (25.01)	36.01 (40.83)	44.94 (35.09)	44.94 (35.09)	32.67 (26.07)
state has PSL	8.375 (27.57)	8.375 (27.57)	1.031 (0.734)	2.964 (2.381)	-	-
days since National Emergency Declaration X post National Emergency Declaration		3.545*** (0.893)	3.545*** (0.893)	3.043** (1.220)	3.043** (1.220)	4.988* (2.558)
state has PSL X days since National Emergency Declaration				0.129 (0.110)	0.129 (0.110)	0.136* (0.0805)
state has PSL X days since National Emergency Declaration X post national Emergency Declaration				1.702 (1.490)	1.702 (1.490)	2.995 (2.933)
Observations	7,928,292	7,928,292	7,928,292	7,928,292	7,928,292	7,928,058
Adjusted R-squared	0.078	0.084	0.084	0.085	0.429	0.717
Census Block Group f.e.					X	X
County X Month Of Year X Day Of Week f.e.						X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for cases are The COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts from February 12th, 2020-April 13th, 2020. Unit of

observation is a census block group on a day. Results generated using a 60% random subsample of census block groups. “state has PSL” is omitted beginning in column 5 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states.

Table 14
Regression Discontinuity Design: Immediate Effect of National Emergency Declaration
(March 13th, 2020) On New, 14-Day Lead Deaths in County, By State Has PSL
Time Window: February 12th, 2020, 2020 -April 13th, 2020

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
state has PSL X post National Emergency Declaration				-3.686 (4.097)	-3.686 (4.097)	-1.731 (2.837)
days since National Emergency Declaration (running variable)	0.125*** (0.0305)	0.0366 (0.0229)	0.0366 (0.0229)	0.0398 (0.0322)	0.0398 (0.0322)	0.0301 (0.0254)
post National Emergency Declaration (threshold)	3.740 (2.827)	3.554 (2.861)	3.080 (4.195)	4.640 (3.923)	4.640 (3.923)	3.301 (2.736)
state has PSL	0.755 (2.482)	0.755 (2.482)	-0.0358 (0.220)	-0.198 (0.713)	-	-
state has PSL X days since National Emergency Declaration			1.608 (4.655)	-0.0108 (0.0330)	-0.0108 (0.0330)	-0.0142 (0.0270)
days since National Emergency Declaration X post National Emergency Declaration		0.186*** (0.0670)	0.186*** (0.0670)	0.0795 (0.0642)	0.0795 (0.0642)	0.294* (0.165)
state has PSL post National Emergency Declaration X days since National Emergency Declaration				0.363*** (0.107)	0.363*** (0.107)	0.109 (0.180)
Observations	7,928,292	7,928,292	7,928,292	7,928,292	7,928,292	7,928,058
Adjusted R-squared	0.053	0.055	0.056	0.059	0.435	0.715
Census Block Group f.e.					X	
County X Month of Year X Day of Week f.e.						X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for deaths are The COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts from February 12th, 2020-April 13th, 2020. Unit of observation is a census block group on a day. Results generated using a 60% random subsample of census block groups. “state has PSL” is omitted beginning in column 5 because states under PSL laws remained constant during

the study period and census block group fixed effects absorb these static differences across states.

Table 15
Heterogeneous Effects: Effect of National Emergency Declaration (March 13th, 2020) on
Median Home Dwell Time in CBG, by State Has PSL
Time Window: January 1st, 2020 -March 31st, 2020

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
state has PSL	-	-	-	-	-	
post National Emergency Declaration	-	-	-	-	-	-
state has PSL X post National Emergency Declaration	38.43*** (9.727)	38.44*** (9.732)	20.01* (9.966)	37.94*** (9.916)	48.19*** (9.333)	24.25** (11.47)
state has PSL X post National Emergency Declaration X share college-educated			40.68* (23.66)			36.42* (21.73)
state has PSL X post National Emergency Declaration X share non-Hispanic Black				-14.85 (17.24)		-4.974 (13.23)
state has PSL X post National Emergency Declaration X share Hispanic					-100.20** (31.59)	-49.32* (26.86)
Observations	9,850,477	9,850,113	9,850,113	9,850,113	9,850,113	9,850,113
Adjusted R-squared	0.703	0.703	0.706	0.706	0.706	0.707
Controls	X	X	X	X	X	X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from January 1st, 2020-March 31st, 2020. Data source for CBG demographic shares is 2016 American Community Survey 5-Year Estimates. Unit of observation is a census block group on a day. Results generated using a 60% random subsample of census block groups. Controls include census block group fixed effects, date fixed effects, county-month of year-day of week fixed effects, and county linear time trend. “state has PSL” is omitted beginning in column 1 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states. “post National Emergency Declaration” is omitted beginning in column

1 because this was a federal event which is absorbed by day fixed effects.

Table 16
Effect of FFCRA Effective Date (April 1st, 2020) on Median Home Dwell Time in CBG, by
State Has PSL
Time Window: March 1st, 2020 -April 30th, 2020

VARIABLES	(1)	(2)	(3)	(4)
state has no PSL	-84.05*** (25.46)	-	-	-
post FFCRA Effective Date	210.2*** (7.578)	-	-	-
state has no PSL X post FFCRA Effective Date	-24.78*** (8.971)	-24.78*** (8.971)	-4.633 (4.237)	23.40*** (8.315)
Observations	6,607,032	6,607,032	6,607,032	6,607,032
Adjusted R-squared	0.097	0.746	0.751	0.753
Census Block Group f.e.		X	X	X
Date f.e.		X	X	X
County X Month Of Year X Day Of Week f.e.			X	X
County Trend				X
Robust standard errors in parentheses				

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from March 1st, 2020-April 30th, 2020. Unit of observation is a census block group on a day. Results generated using a 50% random subsample of census block groups. “state has no PSL” is omitted beginning in column 2 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states. “post FFCRA effective date” is omitted beginning in column 2 because this was a federal event which is absorbed by day fixed effects.

Table 17
Effect of FFCRA Effective Date (April 1st, 2020) on Median Home Dwell Time in CBG, by
State Has PSL
Time Window: March 13th, 2020-May 31st, 2020

VARIABLES	(1)	(2)	(3)	(4)
state has no PSL	-104.7*** (28.87)	-	-	-
post FFCRA effective date	69.34*** (4.730)	-	-	-
state has no PSL X post FFCRA effective date	-1.048 (5.666)	-1.048 (5.666)	-4.633 (4.237)	-1.510 (4.975)
Observations	8,556,648	8,556,648	8,556,648	8,556,648
Adjusted R-squared	0.030	0.726	0.731	0.731
Census Block Group f.e.		X	X	X
Date f.e.		X	X	X
County X Month Of Year X Day Of Week f.e.			X	X
County Trend				X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from March 13th, 2020-May 31st, 2020. Unit of observation is a census block group on a day. Results generated using a 50% random subsample of census block groups. “state has no PSL” is omitted beginning in column 2 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states. “post FFCRA effective date” is omitted beginning in column 2 because this was a federal event which is absorbed by day fixed effects.

Table 18
Effect of FFCRA Effective Date (April 1st, 2020) on Median Home Dwell Time in CBG, by
State Has PSL
Time Window: March 13th, 2020-September 26th, 2020

VARIABLES	(1)	(2)	(3)	(4)
state has no PSL	-104.7*** (28.87)	-	-	-
post FFCRA effective date	-32.90*** (5.410)	-	-	-
state has no PSL X post FFCRA effective date	28.43*** (7.881)	28.43*** (7.881)	-4.633 (4.237)	-8.127* (4.649)
Observations	21,337,464	21,337,464	21,337,464	21,337,464
Adjusted R-squared	0.016	0.656	0.671	0.672
Census Block Group f.e.		X	X	X
Date f.e.		X	X	X
County X Month Of Year X Day Of Week f.e.			X	X
County Trend				X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from March 13th, 2020-September 26th, 2020. Unit of observation is a census block group on a day. Results generated using a 50% random subsample of census block groups. “state has no PSL” is omitted beginning in column 2 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states. “Post FFCRA effective date” is omitted beginning in column 2 because this was a federal event which is absorbed by day fixed effects.

Table 19
Effect of FFCRA Effective Date (April 1st, 2020) on Median Home Dwell Time in CBG, by
State Has PSL
Time Window: March 1st, 2020 -April 30th, 2020

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
state has no PSL	-	-	-	-	-	-	-	-	-
post FFCRA effective date	-	-	-	-	-	-	-	-	-
state has no PSL X post FFCRA effective date	23.59** *	24.89** *	25.20** *	23.50** *	21.84**	17.71**	20.63**	12.96	14.60*
	(8.127)	(7.236)	(7.547)	(8.057)	(8.501)	(8.013)	(7.850)	(7.848)	(7.869)
7-days lagged cumulative cases in state		-0.00057 0***							-0.0008 15
		(0.00017 8)							(0.0007 13)
7-days lagged cumulative deaths in state			-0.00684 ***						0.00443
			(0.00188)						(0.0078 6)
state closes schools				3.078					2.827
				(5.161)					(3.907)
state declares state of emergency					-15.32* *				-9.341*
					(5.952)				(5.451)
state closes non-essential biz						13.92** *			-3.389
						(4.984)			(6.061)
state closes restaurants							13.43** *		8.511*
							(4.789)		(4.908)
state institutes stay-at-home order								22.18** *	17.17** *
								(5.522)	(5.297)
Observations	7,928,2	7,928,29	7,928,29	7,928,2	7,928,2	7,928,2	7,928,29	7,928,29	7,928,29

	92	2	2	92	92	92	2	2	2
Adjusted R-squared	0.754	0.754	0.754	0.754	0.754	0.754	0.754	0.754	0.754
Controls	X	X	X	X	X	X	X	X	X

Robust standard errors in
parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from March 1st, 2020-April 30th, 2020. Data source for stay-at-home orders, state of emergency in states, school closures, nonessential business closures, and restaurant closures is Boston University COVID-19 US State Policy Database. Data sources for cases and deaths are The COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts. Unit of observation is a census block group on a day. Results generated using a 60% random subsample of census block groups. Controls include census block group fixed effects, date fixed effects, county-month of year-day of week fixed effects, and county linear time trend. “state has no PSL” is omitted beginning in column 1 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states. “post FFCRA effective date” is omitted beginning in column 1 because this was a federal event which is absorbed by day fixed effects.

Table 20
Heterogeneous Effects: Effect of FFCRA Effective Date (April 1st, 2020) on Median Home
Dwell Time in CBG, by State Has PSL
Time Window: March 1st, 2020 -April 30th, 2020

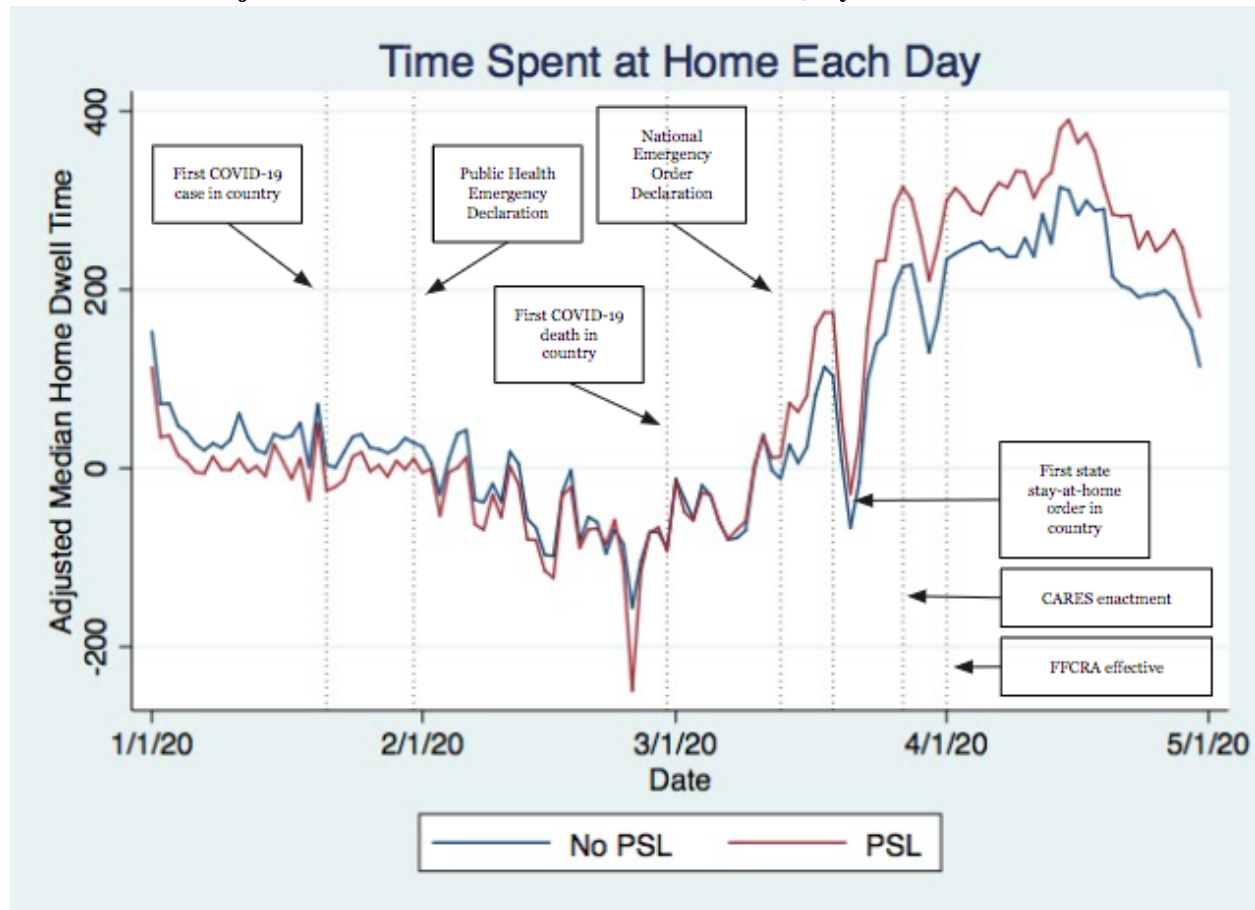
VARIABLES	(1)	(2)	(3)	(4)	(5)
post FFCRA effective date	-	-	-	-	-
state has no PSL	-	-	-	-	-
state has no PSL X post FFCRA effective date	23.38*** (8.306)	23.38*** (8.306)	16.37** (7.510)	20.56** (8.990)	23.21** (9.624)
state has no PSL X post FFCRA effective date X share college-educated			7.897 (16.26)		
state has no PSL X post FFCRA effective date X share non-Hispanic Black				8.655 (16.39)	
state has no PSL X post FFCRA effective date X share Hispanic					41.54* (23.28)
Observations	6,603,067	6,602,823	6,602,823	6,602,823	6,602,823
Adjusted R-squared	0.753	0.753	0.756	0.754	0.753
Controls	X	X	X	X	X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

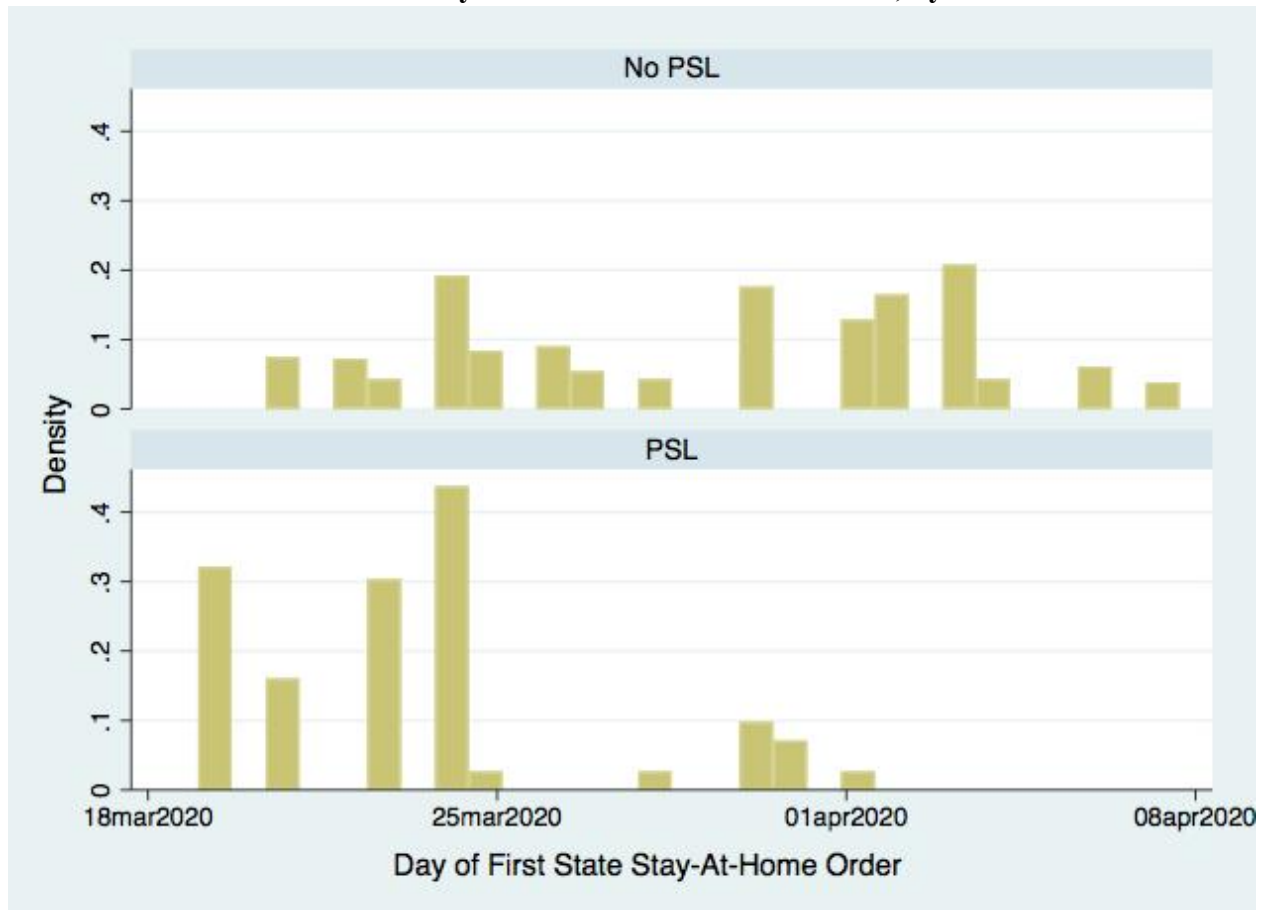
Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from March 1st, 2020-April 30th, 2020. Data source for CBG demographic shares is 2016 American Community Survey 5-Year Estimates. Unit of observation is a census block group on a day. Results generated using a 60% random subsample of census block groups. Controls include census block group fixed effects, date fixed effects, county-month of year-day of week fixed effects, and county linear time trend. “state has no PSL” is omitted beginning in column 1 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states. “post FFCRA effective date” is omitted beginning in column 1 because this was a federal event which is absorbed by day fixed effects.

Figure 1
Adjusted Median Home Dwell Time in CBG, By State Has PSL



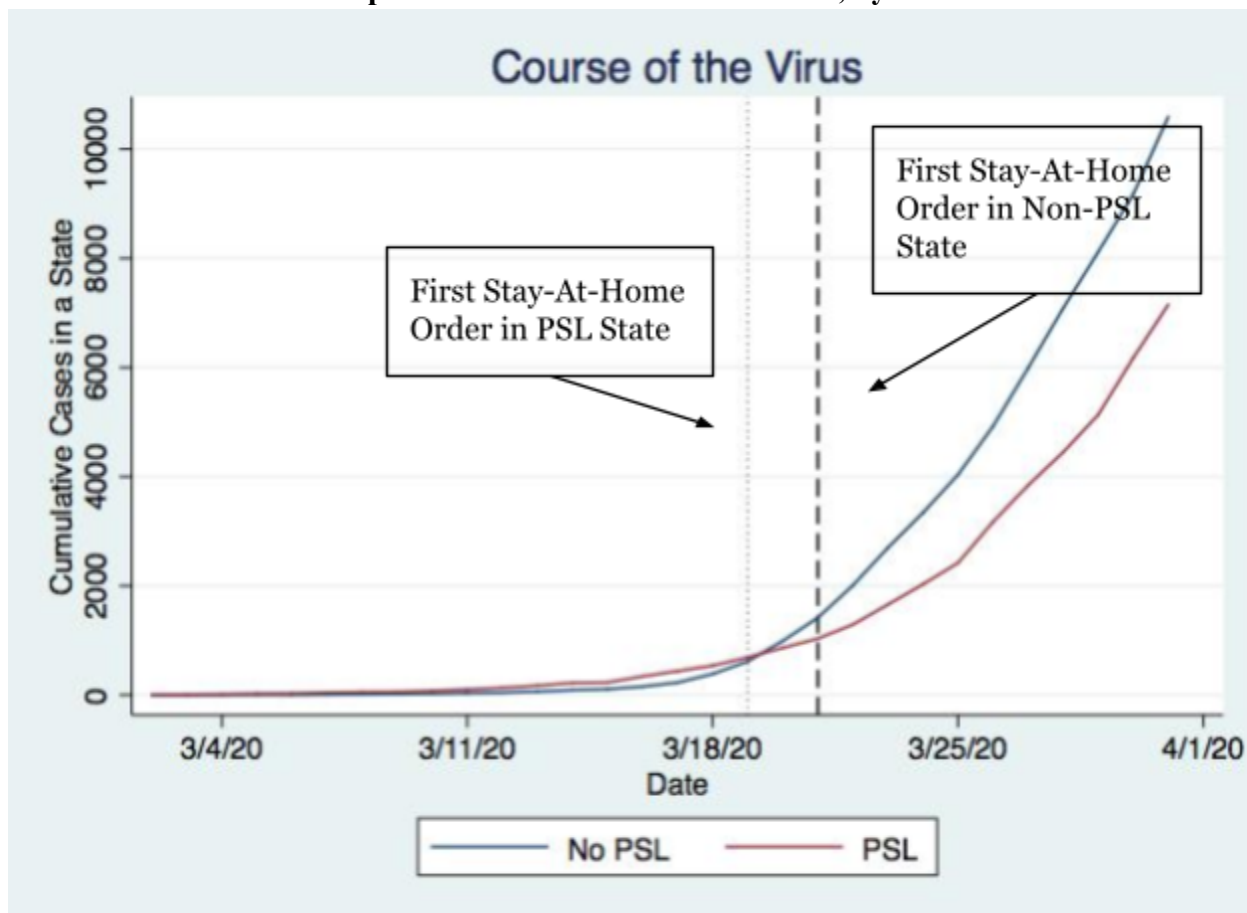
Notes: Data source is SafeGraph Social Distancing Metrics (v2) from January 1st, 2020-April 30th, 2020. Unit of observation is a census block group on a day. Figure generated using a 60% random subsample of census block groups. “Adjusted Median Home Dwell Time” is equivalent to median home dwell time which has been detrended by county-month of year-day of week fixed effects. Dashed lines denote following events in order: (1) First reported COVID-19 case in country, January 21st, 2020 (2) Public Health Emergency Declaration, January 31st, 2020 (3) First reported COVID-19 death in country, February 29th, 2020 (4) National Emergency Declaration, March 13th, 2020 (5) First stay-at-home order in country, March 19th, 2020 (6) CARES Act enactment, March 27th, 2020 (7) FFCRA effective date, April 1st, 2020

Figure 2
Effective Date of First Stay-At-Home Order in Each State, by State Has PSL



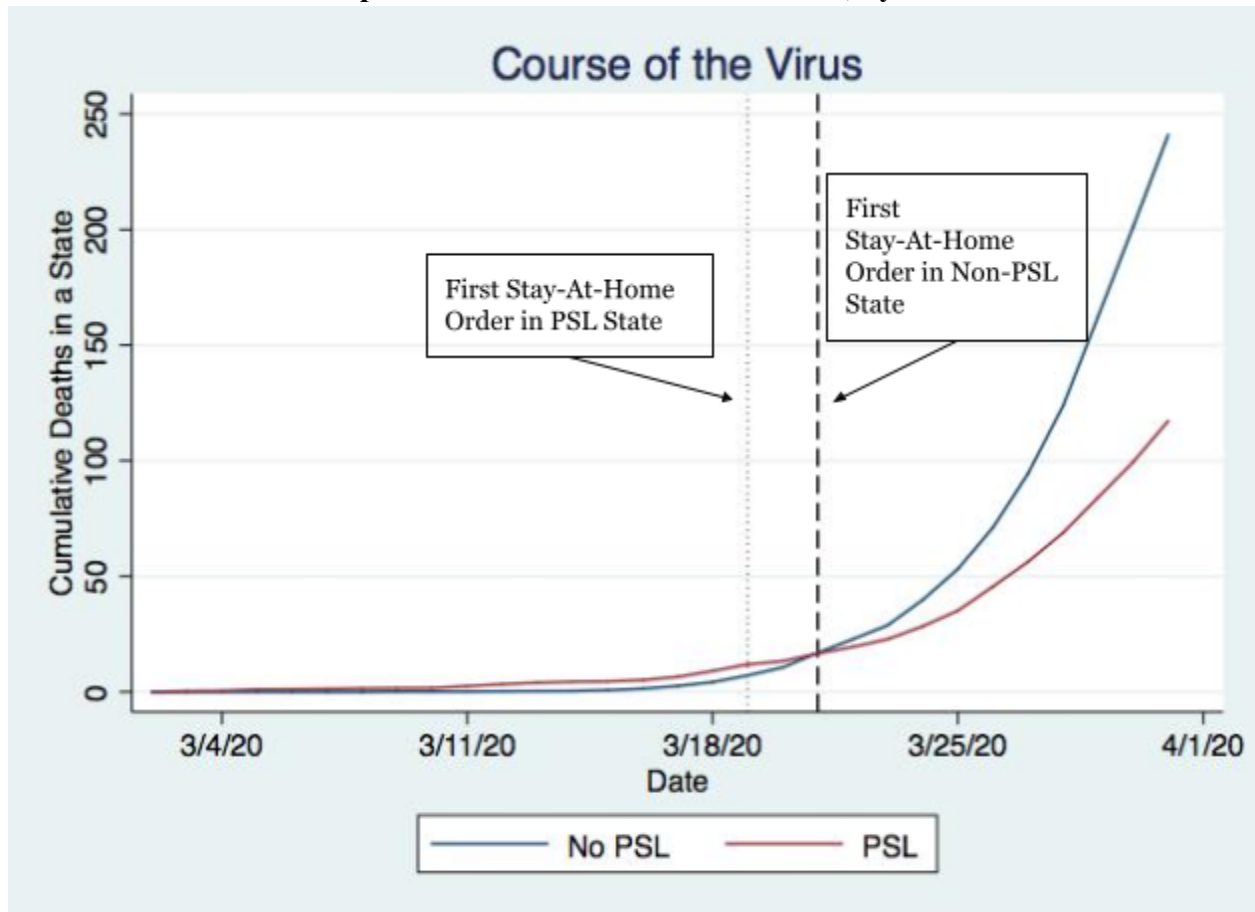
Notes: Data source is Boston University School of Public Health COVID-19 US State Policy Database. Observations are states. Frequencies are normalized so that they sum to 1.

Figure 3
Cumulative Reported COVID-19 Cases in a State, by State Has PSL



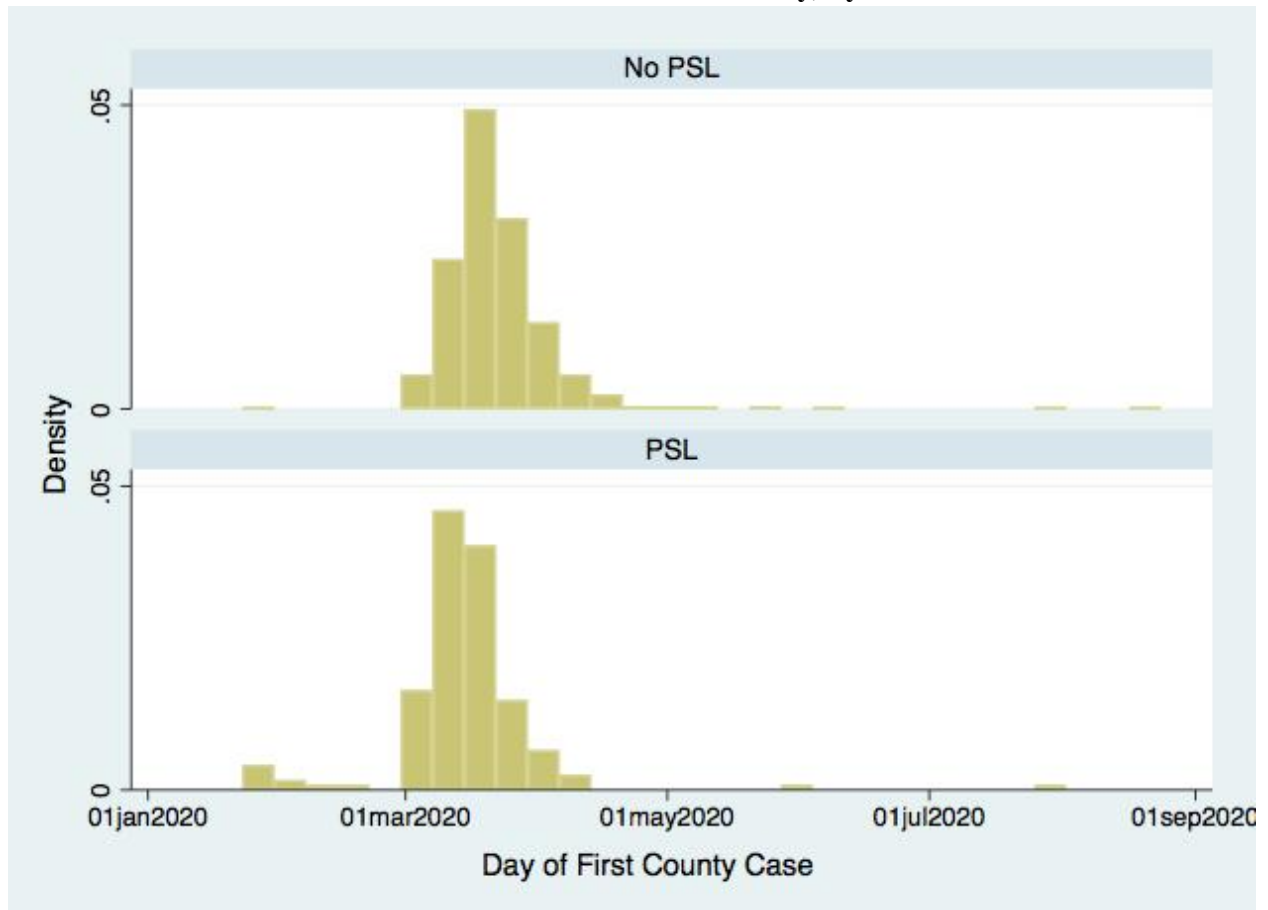
Notes: Data sources are COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts. COVID-19 cases aggregated up to state-level. Dotted line shows the date of first stay-at-home order in a PSL state (California) on March 19th, 2020. Dashed line shows the date of first stay-at-home order in a non-PSL state (Illinois) on March 21st, 2020.

Figure 4
Cumulative Reported COVID-19 Deaths in a State, by State Has PSL



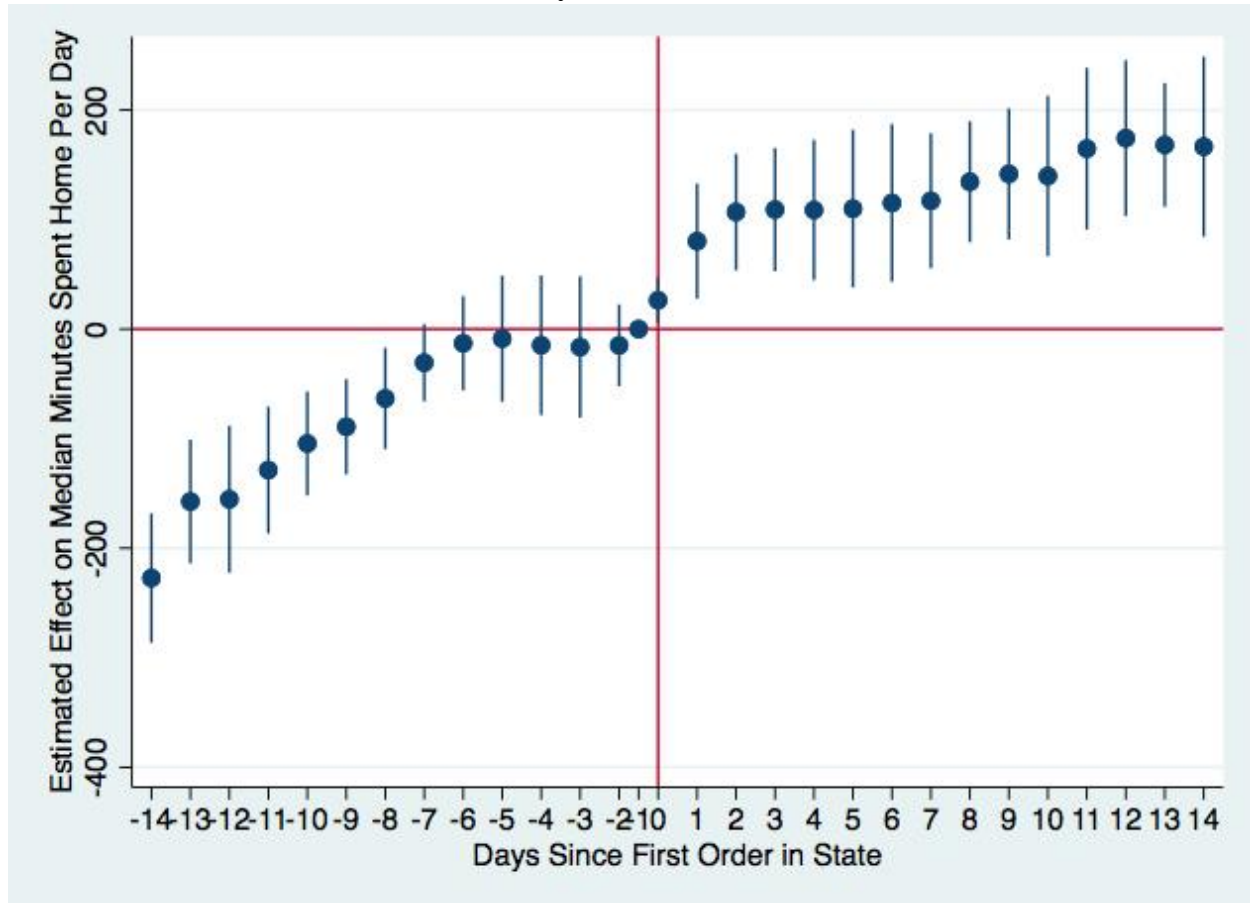
Notes: Data sources are COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts. COVID-19 deaths aggregated up to state-level. Dotted line shows the date of first stay-at-home order in a PSL state (California) on March 19th, 2020. Dashed line shows the date of first stay-at-home order in a non-PSL state (Illinois) on March 21st, 2020.

Figure 5
Date of First COVID-19 Case in Each County, by State Has PSL



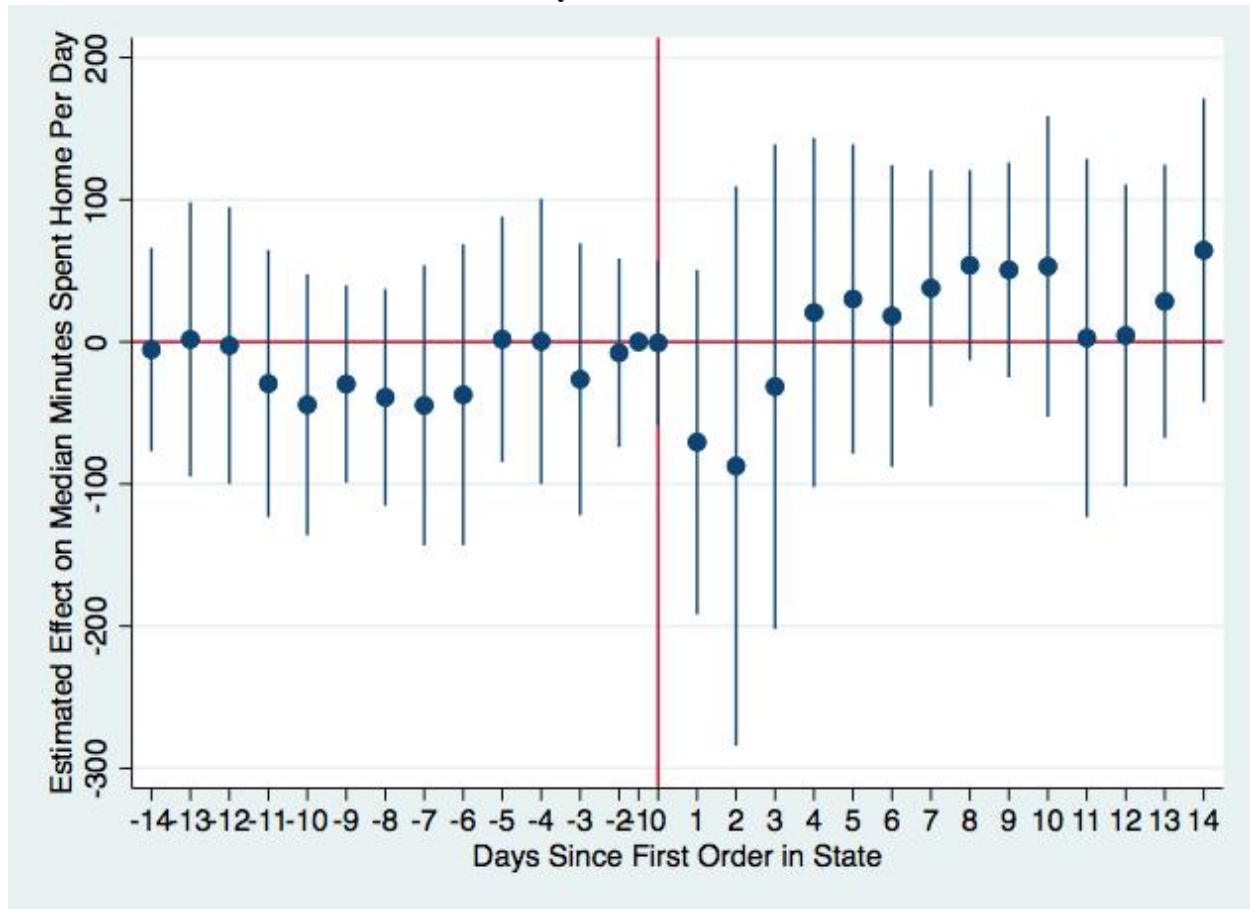
Notes: Data source is SafeGraph Social Distancing Metrics (v2). Observations are counties. Frequencies are normalized so that they sum to 1.

Figure 6
Event Study Estimates for Median Minutes Spent At Home For Non-PSL Places
Event: First Stay-At-Home Order in State



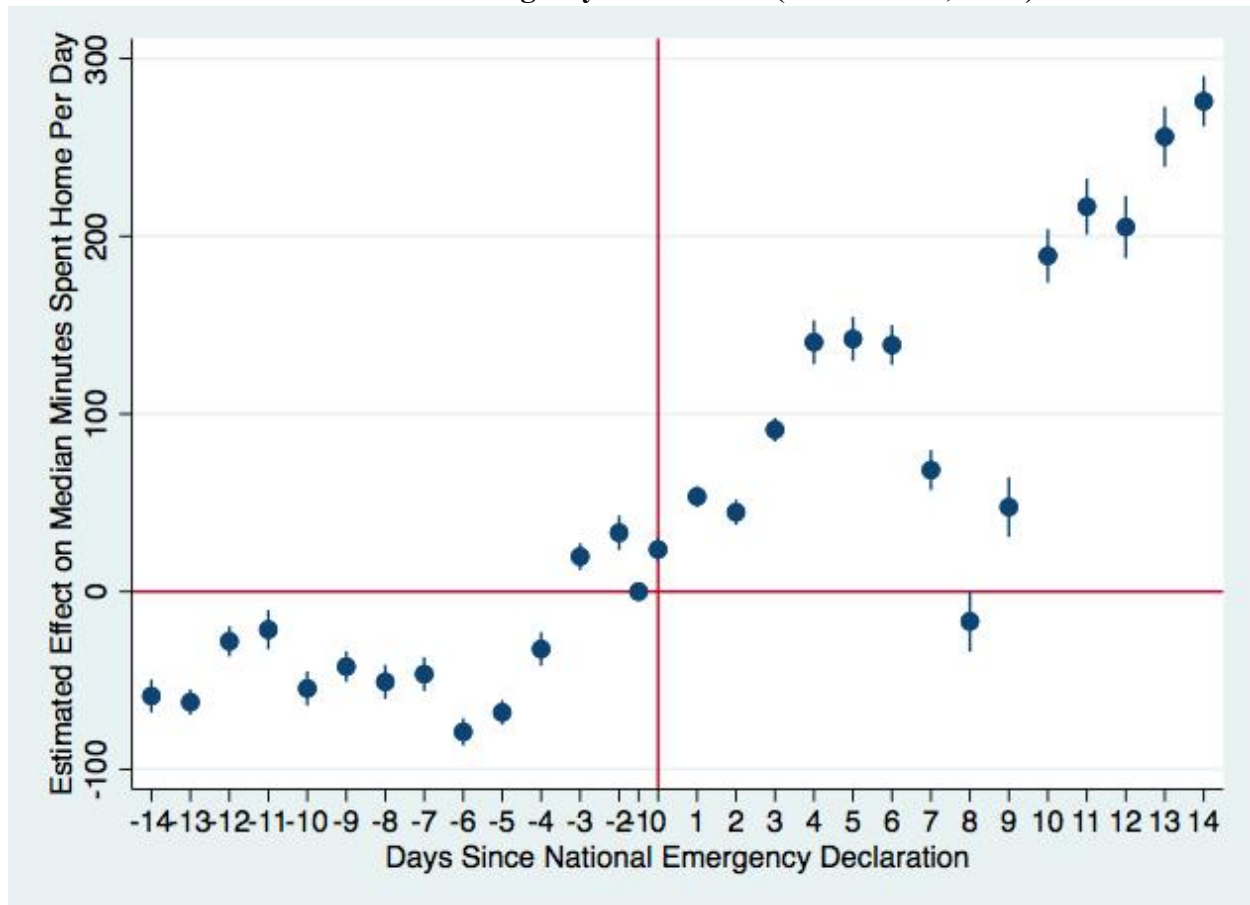
Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from January 1st, 2020-April 30th, 2020. Data source for stay-at-home orders is Boston University COVID-19 US State Policy Database. Unit of observation is a census block group on a day. The omitted category is the day before the date of first order in state. Figure generated using 20% subsample of CBGs. I control for CBG and county-day of week fixed effects, as well as county linear time trends.

Figure 7
Event Study Estimates for Median Minutes Spent At Home For PSL Places Relative to
Non-PSL Places
Event: First Stay-At-Home Order in State



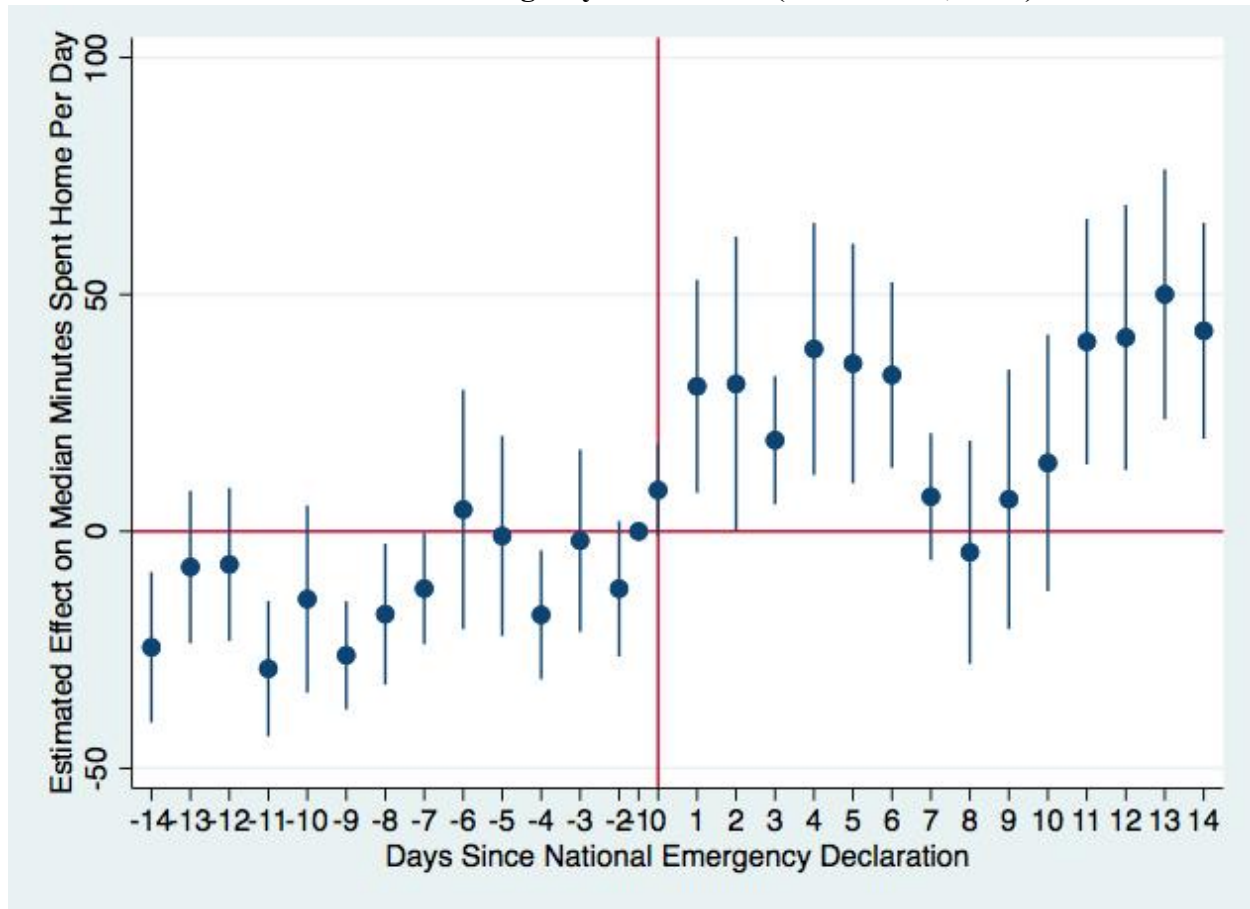
Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from January 1st, 2020-April 30th, 2020. Data source for stay-at-home orders is Boston University COVID-19 US State Policy Database. Unit of observation is a census block group on a day. The omitted category is the day before the date of first order in state. Figure generated using 20% subsample of CBGs. I control for CBG and county-day of week fixed effects, as well as county linear time trends.

Figure 8
Event Study Estimates for Median Minutes Spent At Home For Non-PSL Places
Event: National Emergency Declaration (March 13th, 2020)



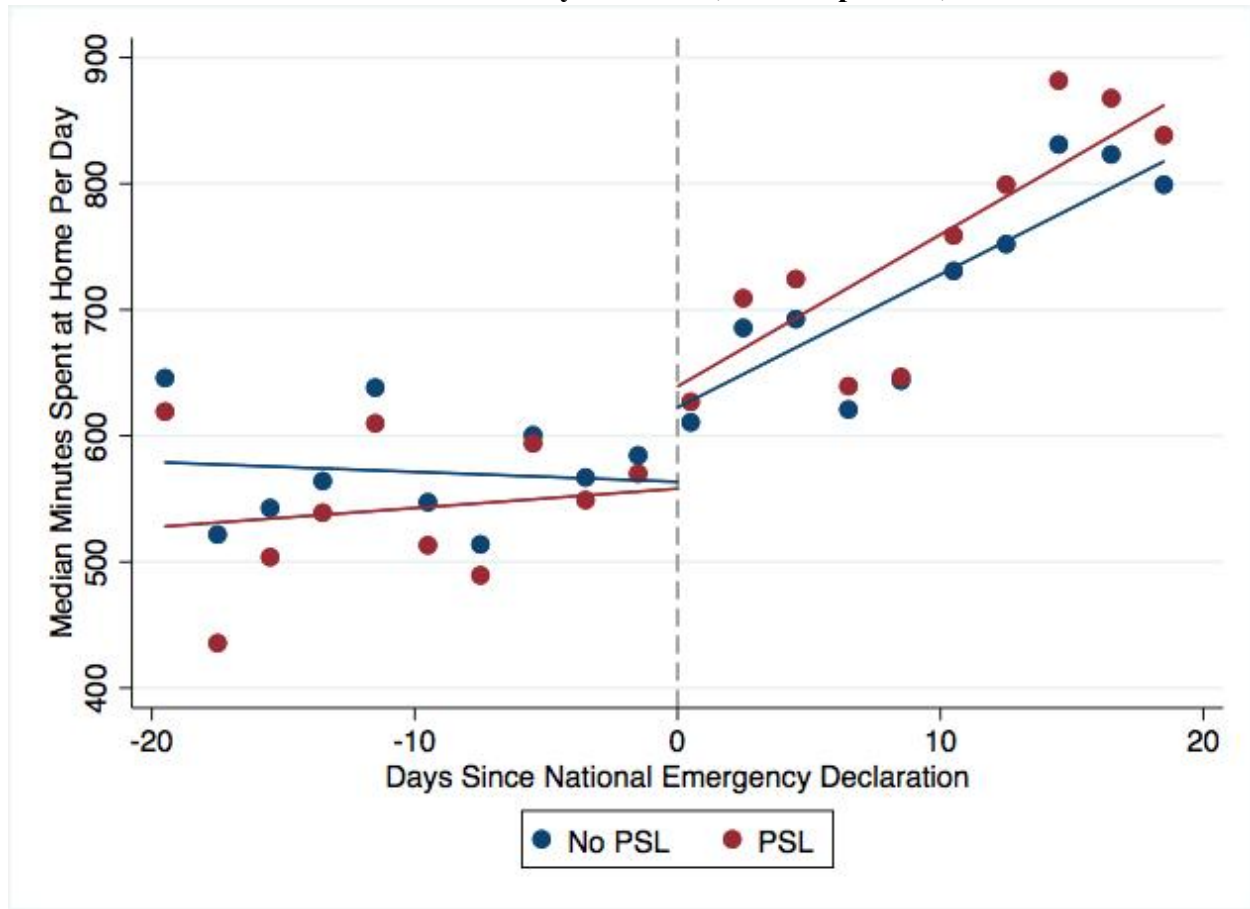
Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from January 1st, 2020-March 31st, 2020. Unit of observation is a census block group on a day. The omitted category is the day before the National Emergency Declaration. Figure generated using 20% subsample of CBGs. I control for CBG and county-day of week fixed effects, as well as county linear time trends.

Figure 9
Event Study Estimates for Median Minutes Spent At Home For PSL Places Relative to
Non-PSL Places
Event: National Emergency Declaration (March 13th, 2020)



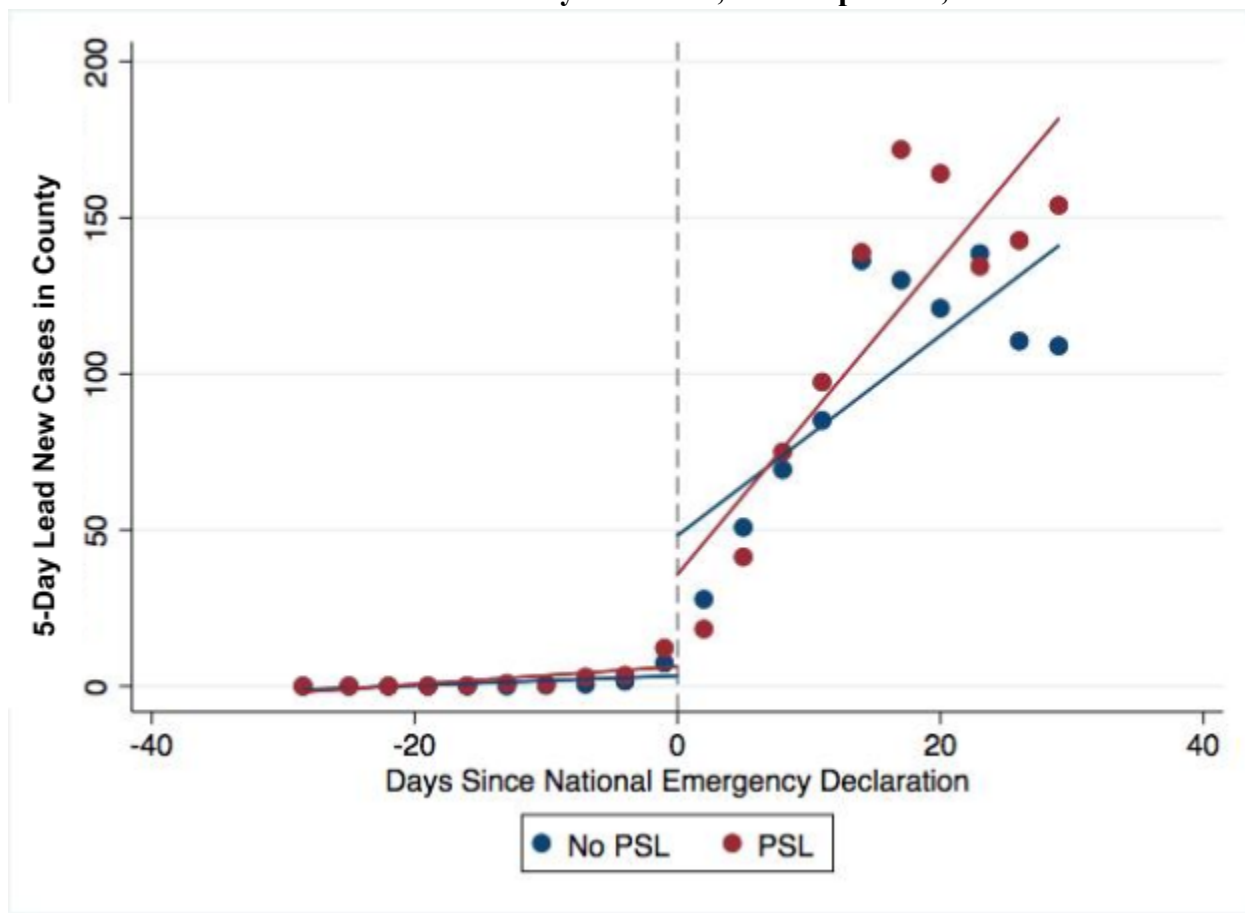
Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from January 1st, 2020-March 31st, 2020. Unit of observation is a census block group on a day. The omitted category is the day before the National Emergency Declaration. Figure generated using 20% subsample of CBGs. I control for CBG and county-day of week fixed effects, as well as county linear time trends.

Figure 10
Regression Discontinuity Plot: Immediate Effect of National Emergency Declaration on
Median Home Dwell Time, By State Has PSL
Time Window: February 20th 2020, 2020 -April 6th, 2020



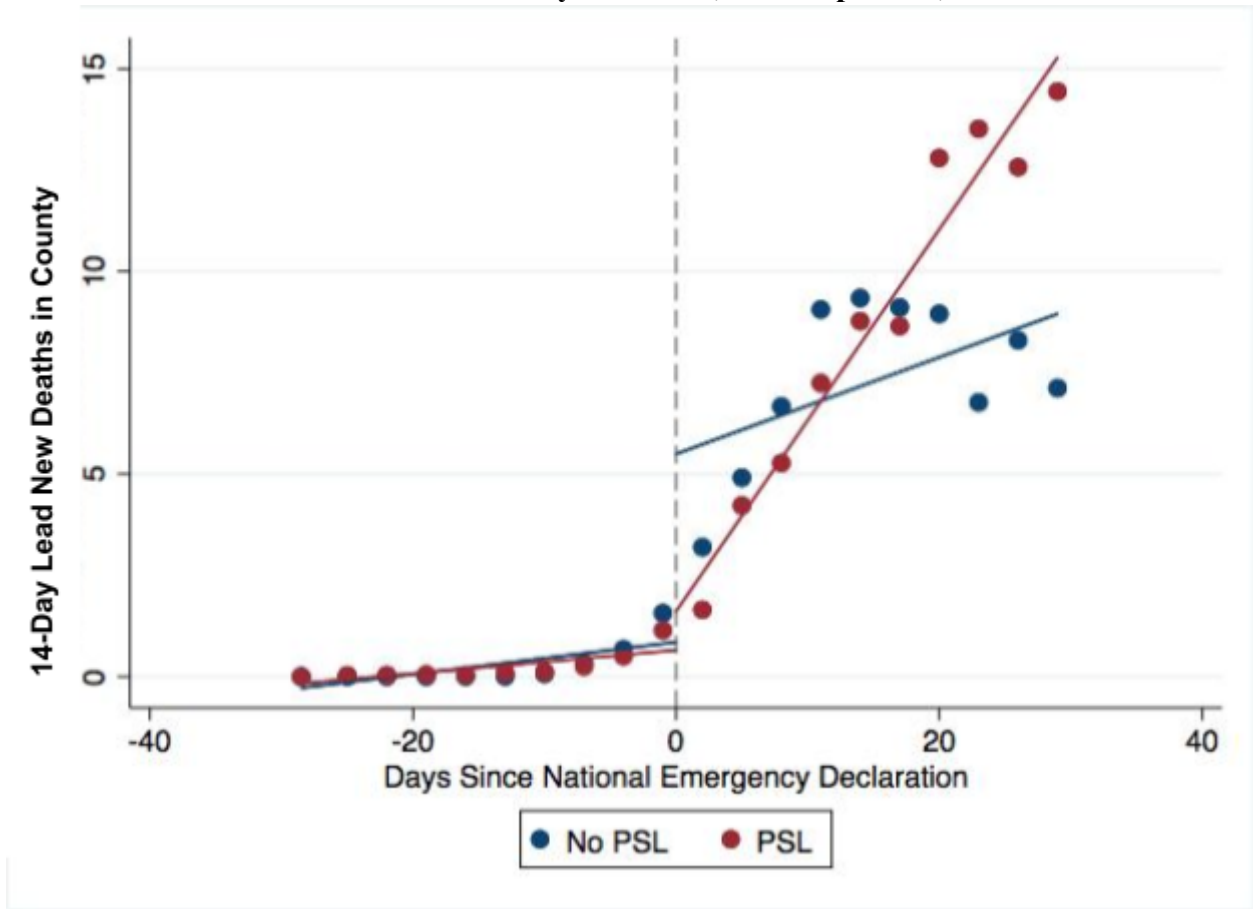
Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from February 20th, 2020-April 6th, 2020. Unit of observation is a census block group on a day. Data binned to 10 equally-sized dots on either side of the threshold. Figure generated using 60% subsample of CBGs. I control for CBG fixed effects.

Figure 11
Regression Discontinuity Plot: Immediate Effect of National Emergency Declaration on
New 5-Day-Lead Cases, By State Has PSL
Time Window: February 20th 2020, 2020 -April 6th, 2020



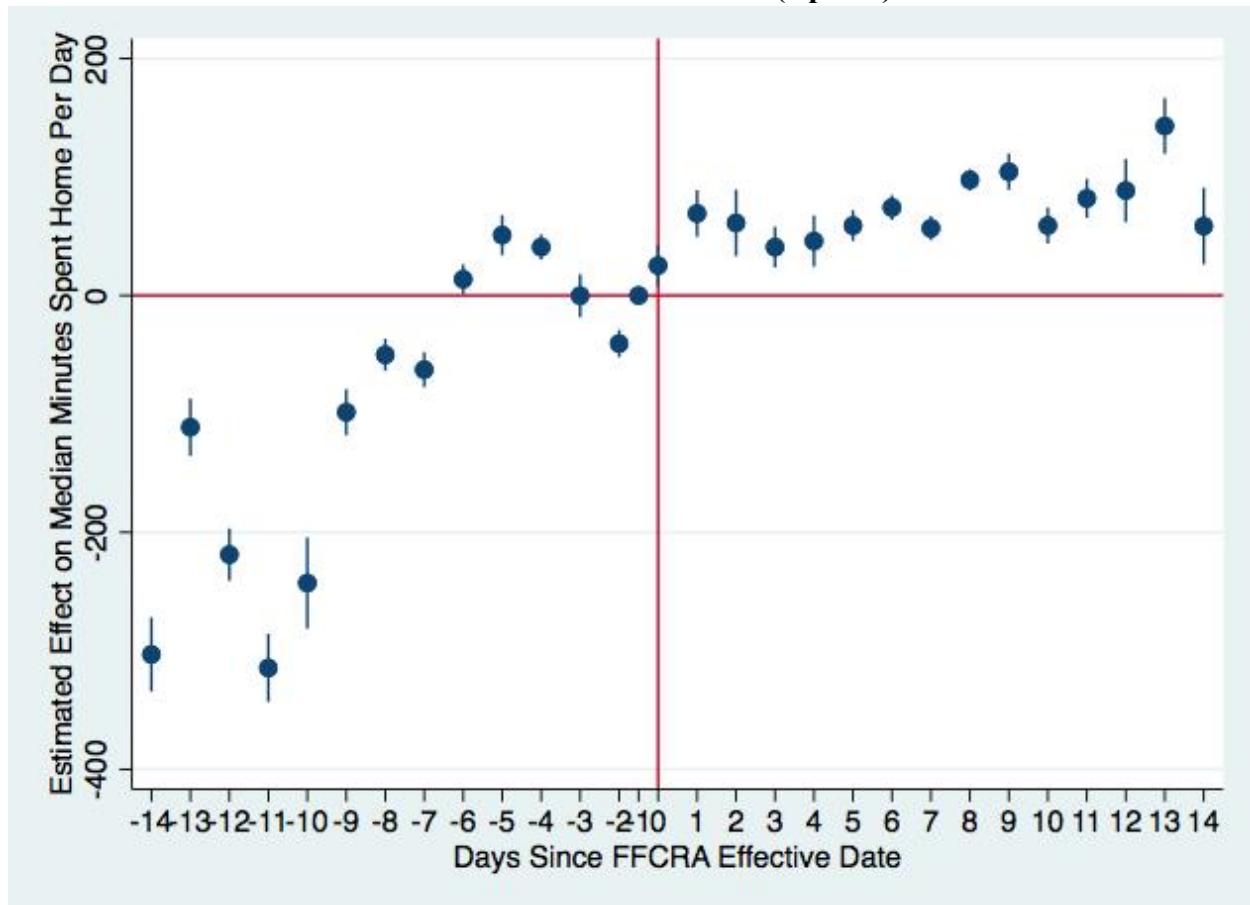
Notes: Data source for case counts are COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts from February 20th, 2020-April 6th, 2020. Unit of observation is a census block group on a day. Data binned to 10 equally-sized dots on either side of the threshold. Figure generated using 60% subsample of CBGs. I control for CBG fixed effects.

Figure 12
Regression Discontinuity Plot: Immediate Effect of National Emergency Declaration on
New 14-Day-Lead Deaths, By State Has PSL
Time Window: February 20th 2020, 2020 -April 6th, 2020



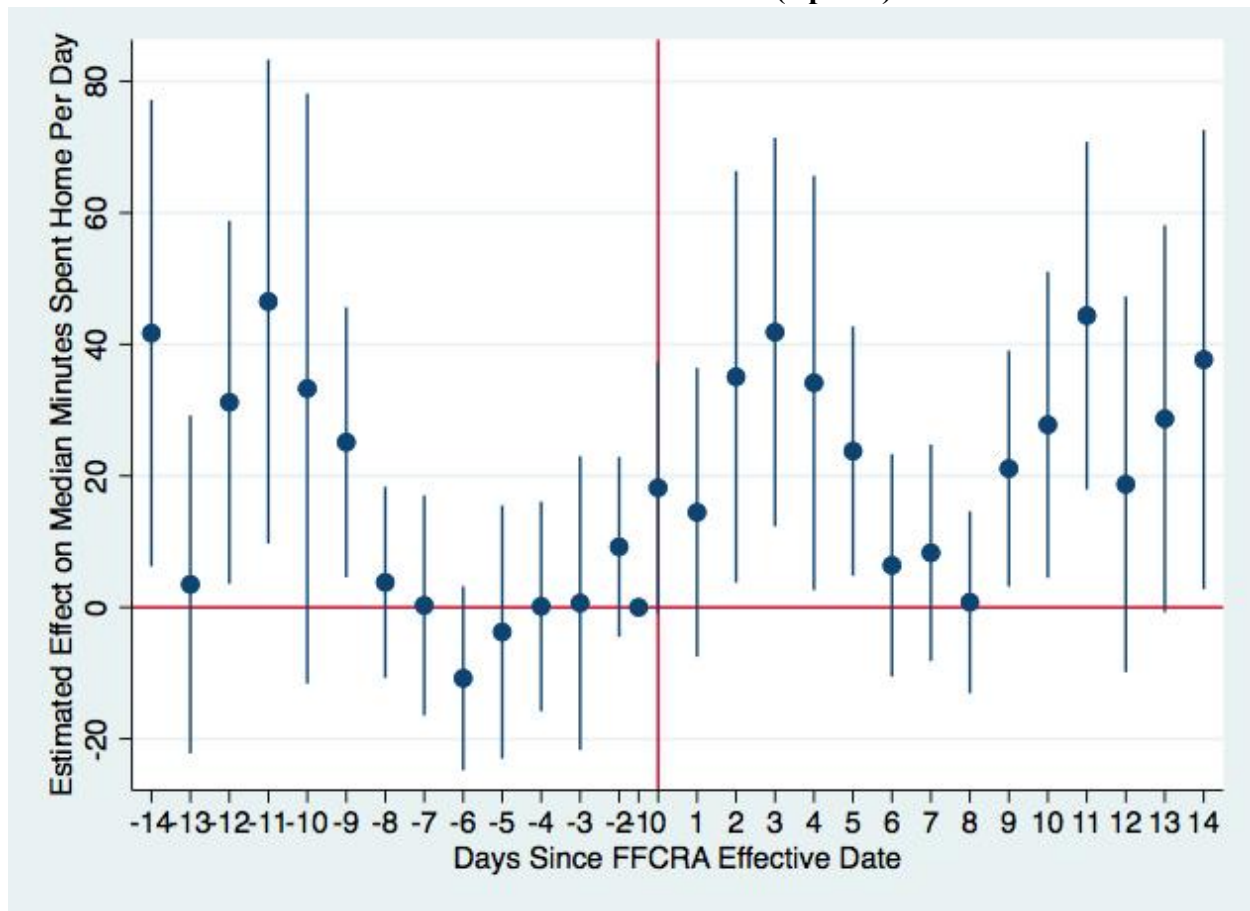
Notes: Data source for death counts are COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts from February 20th, 2020-April 6th, 2020. Unit of observation is a census block group on a day. Data binned to 10 equally-sized dots on either side of the threshold. Figure generated using 60% subsample of CBGs. I control for CBG fixed effects.

Figure 13
Event Study Estimates for Median Minutes Spent At Home For PSL States
Event: FFCRA Effective Date (April 1)



Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from March 1st, 2020-April 30th, 2020. Unit of observation is a census block group on a day. The omitted category is the day before the FFCRA effective date. Figure generated using 20% subsample of CBGs. I control for CBG and county-day of week fixed effects, as well as county linear time trends.

Figure 14
Event Study Estimates for Median Minutes Spent At Home For Non-PSL States Relative to
PSL States
Event: FFCRA Effective Date (April 1)



Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from March 1st, 2020-April 30th, 2020. Unit of observation is a census block group on a day. The omitted category is the day before the FFCRA effective date. Figure generated using 20% subsample of CBGs. I control for CBG and county-day of week fixed effects, as well as county linear time trends.

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VIII. Appendix

Appendix Table 1
First Stay-At-Home Orders in Each State, by State Has PSL at Order Start

Jurisdiction	Stay-At-Home Order Start	PSL at Order Start	Jurisdiction	Stay-At-Home Order Start	PSL at Order Start
California	Mar 19, 2020	X	Kansas	Apr 14, 2020	
Illinois	Mar 20, 2020		Maryland	Apr 15, 2020	X
New Jersey	Mar 21, 2020	X	North Carolina	Apr 16, 2020	
New York	Mar 22, 2020		Virginia	Apr 17, 2020	
Connecticut	Mar 23, 2020	X	Arizona	Apr 18, 2020	X
Louisiana	Mar 24, 2020		Washington, D.C	Apr 19, 2020	X
Oregon	Mar 25, 2020	X	Nevada	Apr 20, 2020	X
Washington	Mar 26, 2020	X	Pennsylvania	Apr 21, 2020	
Delaware	Mar 27, 2020		Tennessee	Apr 22, 2020	
Indiana	Mar 28, 2020		Maine	Apr 23, 2020	
Massachusetts	Mar 29, 2020	X	Texas	Apr 24, 2020	
Michigan	Mar 30, 2020	X	Florida	Apr 25, 2020	
New Mexico	Mar 31, 2020		Georgia	Apr 26, 2020	
Ohio	Apr 1, 2020		Mississippi	Apr 27, 2020	
West Virginia	Apr 2, 2020		Alabama	Apr 28, 2020	
Hawaii	Apr 3, 2020		Missouri	Apr 29, 2020	
Idaho	Apr 4, 2020		South Carolina	Apr 30, 2020	
Vermont	Apr 5, 2020	X	Arkansas	n/a	
Wisconsin	Apr 6, 2020		Iowa	n/a	
Colorado	Apr 7, 2020		Nebraska	n/a	
Kentucky	Apr 8, 2020		North Dakota	n/a	
Minnesota	Apr 9, 2020		Oklahoma	n/a	
Alaska	Apr 10, 2020		South Dakota	n/a	
Montana	Apr 11, 2020		Utah	n/a	

New Hampshire Apr 12, 2020

Wyoming

n/a

Rhode Island Apr 13, 2020

X

Notes: Data source for stay-at-home order dates is the COVID-19 US State Policy Database from the Boston University School of Public Health. Stay-At-Home Order Start is n/a if the state did not enact an explicit stay-at-home order.

Appendix Table 2
Regression Discontinuity Design: Immediate Effect of National Emergency Declaration
(March 13th, 2020) On New, 14-Day-Lead Cases in County, By State Has PSL
Time Window: February 12th 2020, 2020 -April 13th, 2020

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
state has PSL X post National Emergency Declaration			40.82 (52.36)	1.036 (46.24)	1.036 (46.24)	9.000 (42.90)
days since National Emergency Declaration (running variable)	1.320*** (0.458)	2.157** (1.007)	2.157** (1.007)	2.192 (1.423)	2.192 (1.423)	1.635 (1.036)
post National Emergency Declaration (threshold)	71.16** (28.93)	72.92** (30.85)	60.88 (45.54)	72.61* (43.23)	72.61* (43.23)	66.59 (40.89)
state has PSL	19.82 (35.16)	19.82 (35.16)	-0.256 (10.09)	-2.022 (31.68)	-	-
days since National Emergency Declaration X post National Emergency Declaration		-1.761 (2.083)	-1.761 (2.083)	-2.586 (2.804)	-2.586 (2.804)	-0.593 (1.767)
state has PSL X days since National Emergency Declaration				-0.118 (1.440)	-0.118 (1.440)	0.216 (1.073)
state has PSL X days since National Emergency Declaration X post National Emergency Declaration				2.798 (3.117)	2.798 (3.117)	1.480 (1.936)
Observations	7,928,292	7,928,292	7,928,292	7,928,292	7,928,292	7,928,058
Adjusted R-squared	0.068	0.069	0.071	0.072	0.517	0.797
Census Block Group f.e.					X	
County X Month of Year X Day of Week f.e.						X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for cases are The COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts from February 12th, 2020-April 13th, 2020. Unit of observation is a census block group on a day. Results generated using a 60% random subsample of census block groups. “state has PSL” is omitted beginning in column 5 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states.

Appendix Table 3
Regression Discontinuity Design: Immediate Effect of National Emergency Declaration
(March 13th, 2020) On New, Same-Day Cases in County, By State Has PSL
Time Window: February 12th 2020, 2020 -April 13th, 2020

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
state has PSL X post National Emergency Declaration			8.087 (47.14)	-7.279 (7.305)	-7.279 (7.305)	1.253 (6.809)
days since National Emergency Declaration (running variable)	2.736*** (0.929)	0.0412*** (0.0147)	0.0412*** (0.0147)	0.0211* (0.0106)	0.0211* (0.0106)	0.0143 (0.0155)
post National Emergency Declaration (threshold)	-0.266 (5.443)	-5.934 (4.281)	-8.318 (17.06)	-3.788 (5.709)	-3.788 (5.709)	-3.016 (5.647)
state has PSL	4.448 (23.23)	4.448 (23.23)	0.471* (0.265)	1.494* (0.865)	-	-
days since National Emergency Declaration X post National Emergency Declaration		5.668*** (1.941)	5.668*** (1.941)	5.415* (2.737)	5.415* (2.737)	5.181* (2.720)
state has PSL X days since National Emergency Declaration				0.0682* (0.0400)	0.0682* (0.0400)	0.0944*** (0.0330)
state has PSL X days since National Emergency Declaration X post National Emergency Declaration				0.857 (2.885)	0.857 (2.885)	-0.160 (2.770)
Observations	7,928,292	7,928,292	7,928,292	7,928,292	7,928,292	7,928,058
Adjusted R-squared	0.074	0.094	0.094	0.094	0.373	0.807
Census Block Group f.e.						

County X Month of Year X Day of
Week f.e.

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: Data source for cases are The COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts from February 12th, 2020-April 13th, 2020. Unit of observation is a census block group on a day. Results generated using a 60% random subsample of census block groups. “state has PSL” is omitted beginning in column 5 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states.

Appendix Table 4
Regression Discontinuity Design: Immediate Effect of National Emergency Declaration
(March 13th, 2020) On New, Same-Day Deaths in County, By State Has PSL
Time Window: February 12th 2020, 2020 -April 13th, 2020

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
days since National Emergency Declaration (running variable)	0.185* (0.0948)	0.000735 (0.000741)	0.000735 (0.000741)	0 (0)	0 (0)	0.00364 (0.00252)
post National Emergency Declaration	-1.938** (0.891)	-2.325** (1.090)	-2.826 (3.082)	-2.350 (0)	-2.350 (0)	-1.579 (1.039)
days since National Emergency Declaration X post National Emergency Declaration		0.387* (0.199)	0.387* (0.199)	0.404 (0)	0.404 (0)	0.280 (0.201)
state has PSL	-0.330 (1.422)	-0.330 (1.422)	0.0192 (0.0197)	0.0566 (0)	-	-
state has PSL X days since National Emergency Declaration			-0.711 (0.289)	0.00249 (0)	0.00249 (0)	-0.00220 (0.00325)
state has PSL X post National Emergency Declaration				0.0867 (0)	0.0867 (0)	0.280 (1.110)
state has PSL X days since National Emergency Declaration X post National Emergency Declaration				-0.0563 (0)	-0.0563 (0)	-0.0719 (0.207)
Observations	6,607,032	6,607,032	6,607,032	6,607,032	6,607,032	6,606,702

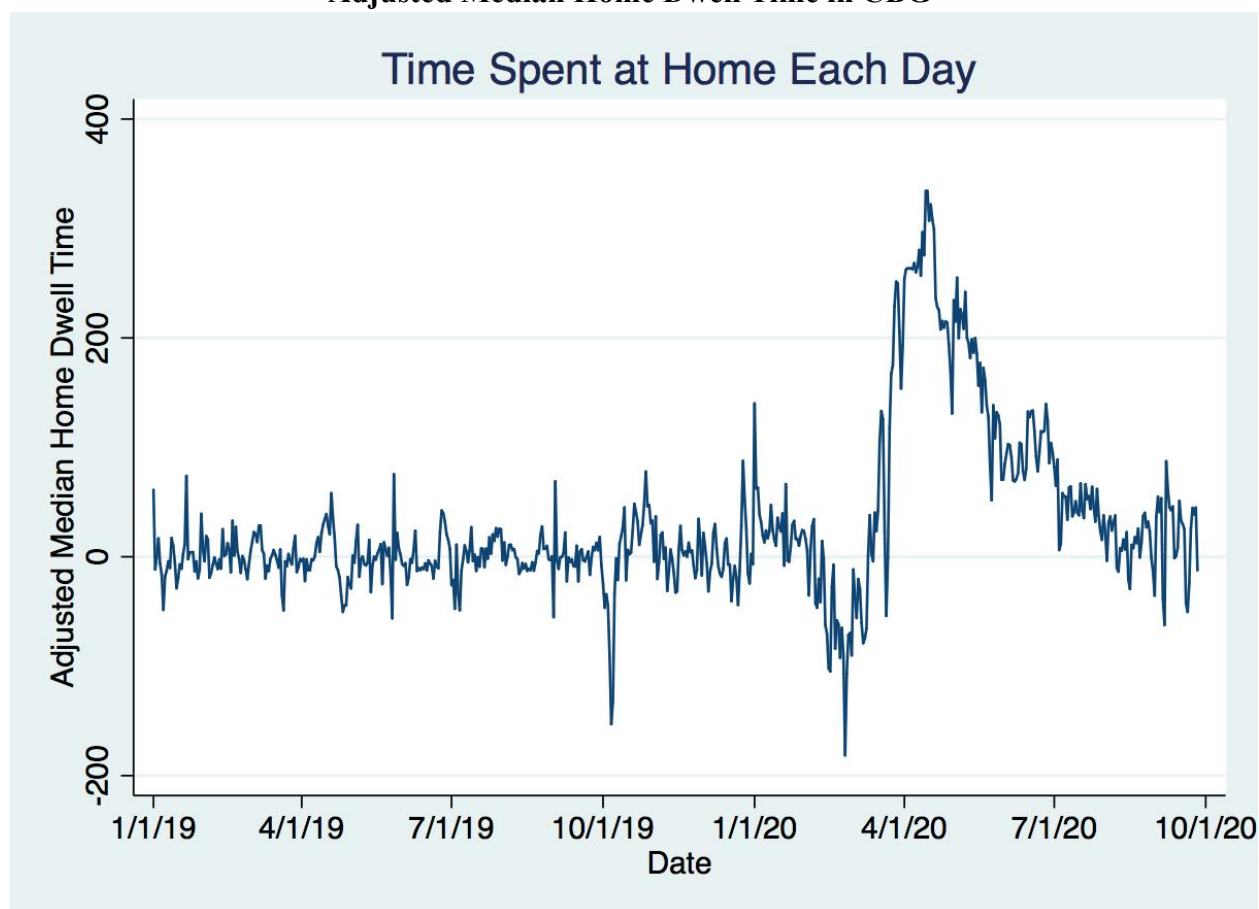
Adjusted R-squared	0.040	0.059	0.059	0.059	0.282	0.874
Census Block Group f.e.					X	X
County X Month of Year X Day of Week f.e.						X

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

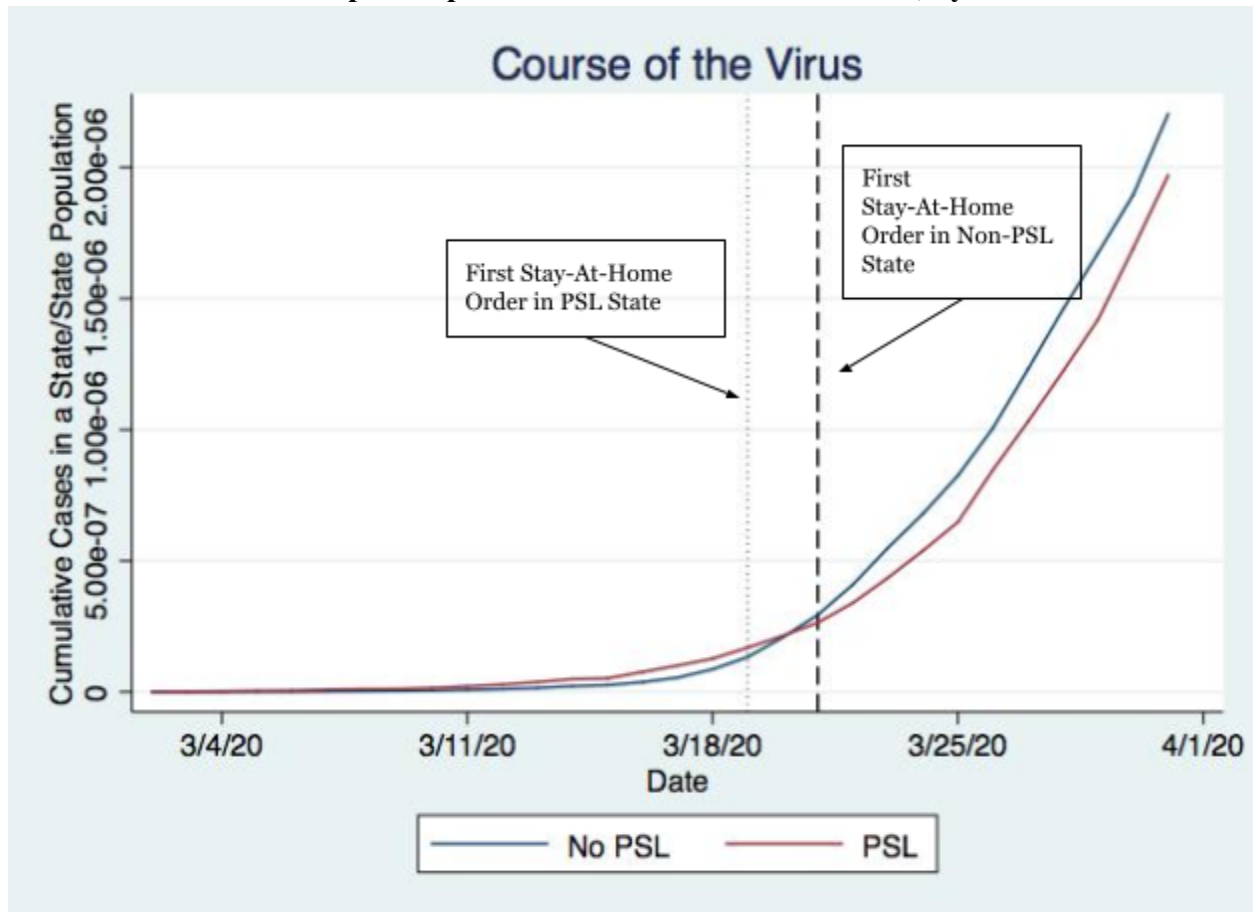
Notes: Data source for deaths are The COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts from February 12th, 2020-April 13th, 2020. Unit of observation is a census block group on a day. Results generated using a 60% random subsample of census block groups. “state has PSL” is omitted beginning in column 5 because states under PSL laws remained constant during the study period and census block group fixed effects absorb these static differences across states. “Zeroed” out standard errors and coefficient values likely due to few new deaths per county in mid-March.

Appendix Figure 1
Adjusted Median Home Dwell Time in CBG



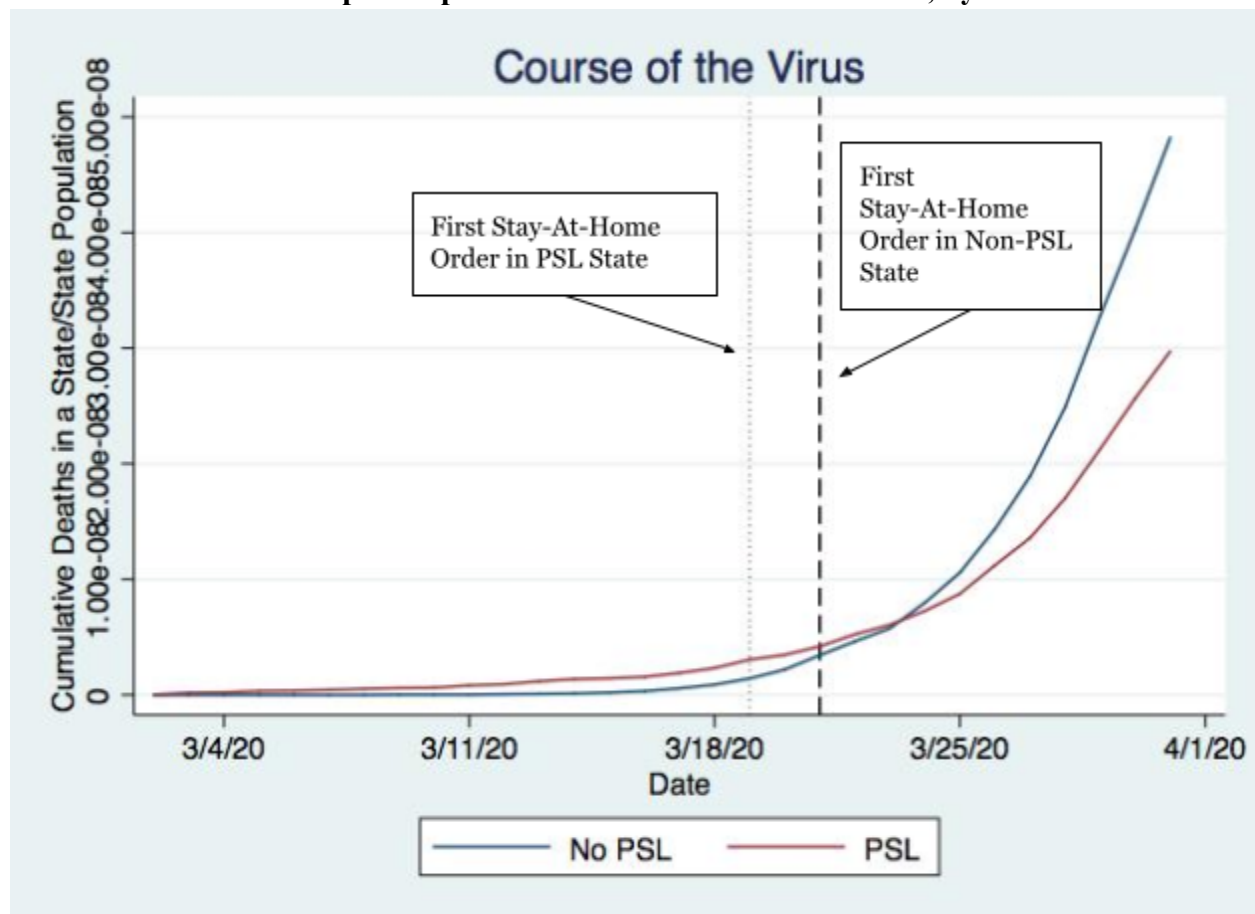
Notes: Data source is SafeGraph Social Distancing Metrics (v2) from January 1st, 2019-September 26th, 2020. Unit of observation is a census block group on a day. Figure generated using a 60% random subsample of census block groups. “Adjusted Median Home Dwell Time” is equivalent to median home dwell time which has been detrended by county-month of year-day of week fixed effects.

Appendix Figure 2
Cumulative Per-Capita Reported COVID-19 Cases in a State, by State Has PSL



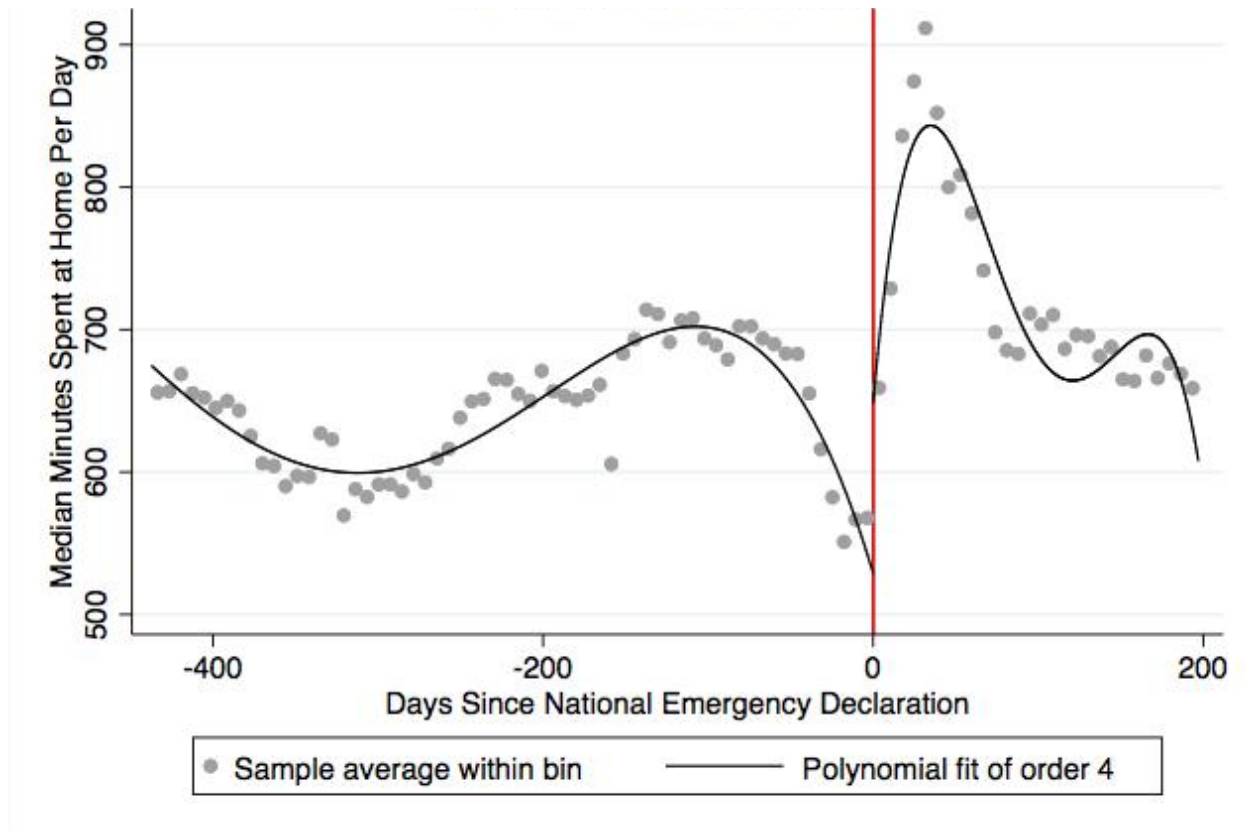
Notes: Data sources are COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts. COVID-19 cases aggregated up to state-level. Dotted line shows the date of first stay-at-home order in a PSL state (California) on March 19th, 2020. Dashed line shows the date of first stay-at-home order in non-PSL state (Illinois) on March 21st, 2020.

Appendix Figure 3
Cumulative Per-Capita Reported COVID-19 Deaths in a State, by State Has PSL



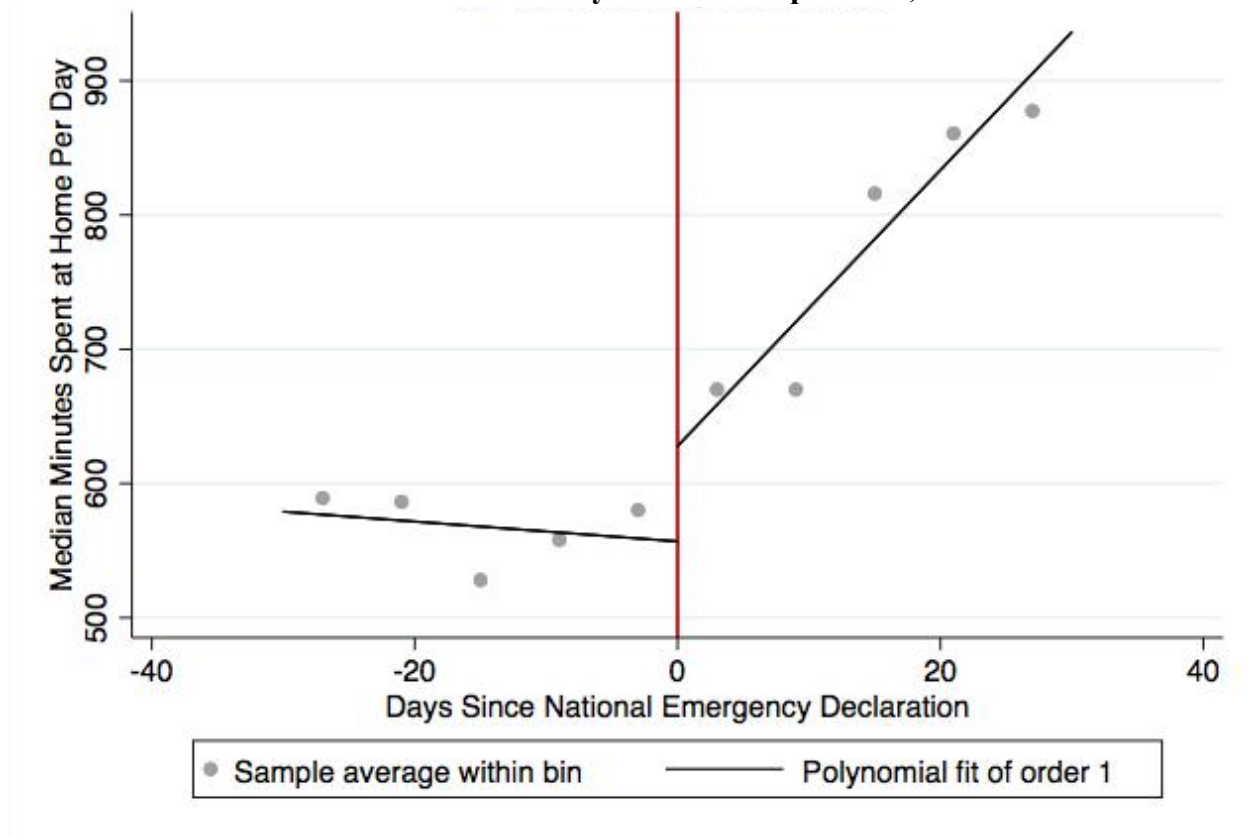
Notes: Data sources are COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University and USAFacts. COVID-19 deaths aggregated up to state-level. Dotted line shows the date of first stay-at-home order in a PSL state (California) on March 19th, 2020. Dashed line shows the date of first stay-at-home order in non-PSL state (Illinois) on March 21st, 2020.

Appendix Figure 4
Regression Discontinuity Plot: Immediate Effect of National Emergency Declaration on
Median Home Dwell Time
Time Window: January 1st, 2019-September 26th, 2020



Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from January 1st, 2019-September 26th, 2020. Unit of observation is a census block group on a day. Data binned to equally-sized dots on either side of the threshold. Figure generated using 60% subsample of CBGs. I control for CBG fixed effects.

Appendix Figure 5
Regression Discontinuity Plot: Immediate Effect of National Emergency Declaration on
Median Home Dwell Time
Time Window: February 12th 2020 -April 13th, 2020



Notes: Data source for median home dwell time is SafeGraph Social Distancing Metrics (v2) from February 12th, 2020-April 13th, 2020. Unit of observation is a census block group on a day. Data binned to equally-sized dots on either side of the threshold. Figure generated using 60% subsample of CBGs. I control for CBG fixed effects.